Science Policy Study—Hearings Volume 11
THE ROLE OF THE BEHAVIORAL AND SOCIAL SCIENCES

HEARINGS

BEFORE THE

TASK FORCE ON SCIENCE POLICY

OF THE

COMMITTEE ON
SCIENCE AND TECHNOLOGY
HOUSE OF REPRESENTATIVES

NINETY-NINTH CONGRESS

FIRST SESSION

SEPTEMBER 17, 18, 19, 1985

[No. 104]

Printed for the use of the
Committee on Science and Technology

U.S. GOVERNMENT PRINTING OFFICE
57-094 O WASHINGTON : 1986

For sale by the Superintendent of Documents, U.S. Government Printing Office
Washington, DC 20402

COMMITTEE ON SCIENCE AND TECHNOLOGY

DON FUQUA, Florida, *Chairman*

ROBERT A. ROE, New Jersey
GEORGE E. BROWN, Jr., California
JAMES H. SCHEUER, New York
MARILYN LLOYD, Tennessee
TIMOTHY E. WIRTH, Colorado
DOUG WALGREN, Pennsylvania
DAN GLICKMAN, Kansas
ROBERT A. YOUNG, Missouri
HAROLD L. VOLKMER, Missouri
BILL NELSON, Florida
STAN LUNDINE, New York
RALPH M. HALL, Texas
DAVE McCURDY, Oklahoma
NORMAN Y. MINETA, California
MICHAEL A. ANDREWS, Texas
BUDDY MacKAY, Florida**
TIM VALENTINE, North Carolina
HARRY M. REID, Nevada
ROBERT G. TORRICELLI, New Jersey
RICK BOUCHER, Virginia
TERRY BRUCE, Illinois
RICHARD H. STALLINGS, Idaho
BART GORDON, Tennessee
JAMES A. TRAFICANT, Jr., Ohio

MANUEL LUJAN, Jr., New Mexico*
ROBERT S. WALKER, Pennsylvania
F. JAMES SENSENBRENNER, Jr., Wisconsin
CLAUDINE SCHNEIDER, Rhode Island
SHERWOOD L. BOEHLERT, New York
TOM LEWIS, Florida
DON RITTER, Pennsylvania
SID W. MORRISON, Washington
RON PACKARD, California
JAN MEYERS, Kansas
ROBERT C. SMITH, New Hampshire
PAUL B. HENRY, Michigan
HARRIS W. FAWELL, Illinois
WILLIAM W. COBEY, Jr., North Carolina
JOE BARTON, Texas
D. FRENCH SLAUGHTER, Jr., Virginia
DAVID S. MONSON, Utah

HAROLD P. HANSON, *Executive Director*
ROBERT C. KETCHAM, *General Counsel*
REGINA A. DAVIS, *Chief Clerk*
JOYCE GROSS FREIWALD, *Republican Staff Director*

SCIENCE POLICY TASK FORCE

DON FUQUA, Florida, *Chairman*

GEORGE E. BROWN, Jr., California
TIMOTHY E. WIRTH, Colorado
DOUG WALGREN, Pennsylvania
HAROLD L. VOLKMER, Missouri
STAN LUNDINE, New York
NORMAN Y. MINETA, California
HARRY M. REID, Nevada
RICK BOUCHER, Virginia
RICHARD H. STALLINGS, Idaho

MANUEL LUJAN, Jr., New Mexico*
TOM LEWIS, Florida
DON RITTER, Pennsylvania
SID W. MORRISON, Washington
RON PACKARD, California
JAN MEYERS, Kansas
HARRIS W. FAWELL, Illinois
D. FRENCH SLAUGHTER, Jr., Virginia

JOHN D. HOLMFELD, *Study Director*
EZRA HEITOWIT, *Staff Director, Subcommittee on Science, Research and Technology*
ROBERT O'NEILL, *Professional Staff Member*
R. THOMAS WEIMER, *Republican Staff Member*

---

*Ranking Republican Member.
**Serving on Committee on the Budget for 99th Congress.

BEST COPY AVAILABLE

CONTENTS

WITNESSES

September 17, 1985:

Page
- Dr. Herbert A. Simon, Richard King Mellon University Professor of Computer Science and Psychology, Carnegie-Mellon University, Pittsburgh, PA 2
  - Prepared statement 8
- Dr. R. Duncan Luce, Victor S. Thomas Professor of Psychology, Harvard University, Cambridge. MA 16
  - Prepared statement 22
- Dr. Neal J. Smelser, university professor of sociology, University of California at Berkeley, Berkeley, CA 46
  - Prepared statement 52
- Discussion 66
- Questions and answers for the record:
  - Dr. Simon 81
  - Dr. Luce 87
  - Dr. Smelser 92

September 18, 1985:

- Dr. James R. Coleman, Department of Sociology, University of Chicago, Chicago, IL 98
  - Prepared statement 103
- Dr. Joseph Newhouse, head, economics department, Rand Corp., Santa Monica, CA 117
  - Prepared statement 122
- Dr. Walter A. Albers, Jr., head, societal analysis department, General Motors Research Laboratories, Warren, MI 131
  - Prepared statement 135
- Dr. Douglas W. Bray, chairman, Development Dimensions International, Tenafly, NJ 143
  - Prepared statement 148
- Dr. Clark Abt, president and chairman, Abt Associates, Inc., Cambridge, MA 159
  - Prepared statement 164
- Discussion 181
- Questions and answers for the record:
  - Dr. Newhouse 187
  - Dr. Bray 190
  - Dr. Abt 191

September 19, 1985:

- Dr. Francis X. Sutton, consultant, Ford Foundation, New York, NY 196
  - Prepared statement 203
- Dr. Albert Rees, president, the Alfred P. Sloan Foundation, New York, NY 223
  - Prepared statement 227
- Dr. Amitai Etzioni, university professor, George Washington University, Washington, DC and director, the Center for Policy Research, Bethesda, MD 230
  - Prepared statement 235
- Dr. Richard W. Pew, principal scientist, Bolt Beranek & Newman [BBN] Laboratories, Inc., Cambridge, MA 239
  - Prepared statement 244
- Panel discussion 251
- Questions and answers for the record:
  - Dr. Rees 264

Appendix—Additional statement submitted for the record:

- Paul A. Ballonoff, senior economist, Policy Analysis and Research Division, Illinois Commerce Commission, April 25, 1986 267

THE ROLE OF THE BEHAVIORAL AND SOCIAL SCIENCES

TUESDAY, SEPTEMBER 17, 1985

House of Representatives,
Committee on Science and Technology,
Task Force on Science Policy,
Washington, DC.

The task force met, pursuant to call at 10:05 a.m., in room 2318, Rayburn House Office Building, Hon. Doug Walgren (acting chairman of the task force) presiding.

Mr. Walgren. We welcome you all to the Science Policy Task Force hearing on Social and Behavioral Sciences, and let me at the outset ask unanimous consent that there be drafts allowed since we are operating under the more formal rules of the committee. And without objection, that will be the order so there is no problem with drafts.

Today the Science Policy Task Force begins on a special set of three hearings on the Role of Social and Behavioral Sciences. The purpose of this portion of the science policy study is to address the part played by the social and behavioral sciences in Federal science policy.

The hearing today will provide us with a general overview of the social and behavioral sciences, and will highlight the fields of active research. Tomorrow, the second hearing will focus on the utilization of the social and behavioral sciences by industry and Government. On the third day there will be a discussion of the current controversies surrounding Federal funding for these fields, as well as an assessment of the Federal role in applying these disciplines in national policymaking.

Although the task force previously agreed that individual scientific disciplines would not be examined, a special exception was made for the social and behavioral sciences. This exception is warranted in light of the increasing importance of these areas as well as the visibility of Government support for research in these disciplines. Since the social and behavioral sciences, by their very nature, cover all aspects of human endeavor and experience, it is indeed appropriate that the task force examine the direction and scope of these areas of study. We hope, in particular, that these hearings will help the task force come up with recommendations for the future levels of support in these fields.

The witnesses today include three very distinguished individuals. Dr. Herbert Simon of Carnegie-Mellon University in Pittsburgh, as many of you know, a Nobel Laureate, is joined by Dr. R. Duncan

Luce of Harvard University and Dr. Neal Smelser of the University of California at Berkeley.

Let me also acknowledge the presence in the audience of Ms. Genevieve Knezo, the author of the background study done for us by the Library of Congress.

On behalf of the Science Policy Task Force and the Committee on Science and Technology, I want to welcome our witnesses and thank them for their time and efforts.

I recognize Manuel Lujan for any comments at the outset that he might like to make and other members present.

Mr. LUJAN. Thank you, Mr. Chairman.

I agree with you that it is a good thing that we did include as a specific discipline this particular discipline. I have no opening statement. I just want to welcome our distinguished witnesses this morning and hope to be able to at least learn a little bit about the subject this morning. Thank you very much.

Mr. WALGREN. Thank you. Other opening comments? Congressman Brown.

Mr. BROWN. No.

Mr. WALGREN. Congressman Fuqua.

Mr. FUQUA. No, I want to commend you, Mr. Chairman, for convening these hearings. It is a very important aspect of basic research and one that in the eyes of many of us probably has been neglected in recent years. And we have a distinguished panel here, and I am very delighted to be here.

Mr. WALGREN. With that, let me turn to the panel, then, and welcome you again. We appreciate your being here, and let us go forward in the order, first, with Dr. Simon, then Dr. Luce, and then Dr. Smelser. The formal written statements can be incorporated in the record, and you can feel free to take from any text emphasis or elaborations which you would like to make that would underscore points that you feel deserve to be emphasized, and so with that, welcome to the committee.

STATEMENT OF DR. HERBERT A. SIMON, RICHARD KING MELLON UNIVERSITY PROFESSOR OF COMPUTER SCIENCE AND PSYCHOLOGY, CARNEGIE-MELLON UNIVERSITY, PITTSBURGH, PA

Dr. SIMON. My name is Herbert Simon, and I am a professor of computer science and psychology at Carnegie-Mellon University. A few years ago, I chaired a committee for the National Academy of Sciences that reviewed the social and behavioral science programs in the National Science Foundation, and on several previous occasions I have participated in the work of committees of the Academy's Committee on Science and Public Policy that examined the problems and prospects of the social and behavioral sciences in the United States. My own research in recent years lies in the area of cognitive psychology and the neighboring area of artificial intelligence. At an earlier time, I worked in economics and organization theory.

The three of us this morning have worked out a little division of labor to be sure that we take a particular look at areas that each of us is respectively familiar with, and that we don't cover the same ground a large number of times. So in my brief opening remarks, I would like to say a little bit about research in economics, particularly the forecasting aspect of economics, and the more formal aspects of mathematical economics.

I would like to say a little bit about research and decisionmaking, and in particular empirical research, trying to find out how people actually make decisions, and I would like to say a little bit about a new important interdisciplinary field, which has acquired the name of cognitive science.

We often think of the social and behaviorial sciences as new sciences. They really aren't, of course. The first mortality and actuarial tables were prepared 300 years ago, about the same time that Newton was writing his great work. Adam Smith has had his 200th anniversary of the publication of *Wealth of a Nation* celebrated. Vont and Ebinghouse in Germany were establishing the basic procedures of experimental psychology, more than 100 years ago. I helped to celebrate the centennial of Ebinghouse this summer—I guess two of us did, this summer. So we have had the social sciences developing for a substantial period of time.

In many ways we kind of take them for granted. We take for granted the very essential role that information based on social science research plays in our everyday life. When we pick up a newspaper, we find that it is full of social statistics and other sorts of information that have been gathered as a result of social science research, demography. What is our population? How is it distributed by age and so on. Economics—what is our GNP? What is happening to employment, levels of crime in our society? We take those for granted but those kinds of statistics didn't exist 100 years ago, very frequently 50 years ago, certainly not in their present adequate form, even 20 years ago, and they are the product of serious and careful inquiry into how human beings behave, how a society operates.

One reason we don't always think of those as social science is that channels of communication are very diffuse as in the example of newspapers. Most of what we learn about physics we get through the physicist or engineers. Most of what we learn about biology we get from the biologist and the doctors. But somehow in the social sciences we are all our own engineers and we are all our own doctors. You can draw some conclusions perhaps, as to the consequences of that kind of practice, but I think that is a realistic description. So this information is widely diffused in our society, and it is in fact a foundation for our perceptions of reality, the reality that we think about when we make our own decisions, our personal decisions, our family decisions, our decisions, our governmental decisions.

I think most scientists would agree that one way of measuring the wealth of any field of science is in the excitement it attracts. In particular, its ability to attract young people by the research commitment that it offers, and by this score, I think several areas of the social sciences, a number of the areas of social sciences score very well. Today, very bright and able young people are moving, of course, into many nonsocial science fields, high energy physics continues to attract them, molecular biology, but also into the field of economics and a great rate, at present, into the field of cognitive sciences. We are attracting excellent young people because they see there the opportunities to do the kind of exciting scientific work that they want to spend their lives doing.

During my lifetime, and I have been more or less operating in this field for half a century now—during my lifetime there has been a remarkable change in our knowledge about human behavior and human society—a remarkable series of discoveries within the social sciences. You only have to go back to the great volume, *Recent Social Trends*, which was commissioned by President Hoover just before the depression, and was published in 1933, and compare that with the combined knowledge we have about our society today, to see the sort of progress that have taken place—new data.

I have already mentioned several areas where we are continually improving the kind of information we have. A simple matter such as the ability to tape record human verbalization, human speech, and to analyze those tape recordings at leisure, has made an important difference to a number of social sciences, including psychology, linguistics, including others.

Let me get down to some concrete examples for a few moments. I think most of us might agree that during the last decade and a half, economics as a profession has lost a certain amount of face. During the sixties, fifties, we were very sanguine about how well we could regulate and fine tune an economy. I think economists—maybe I ought to say we economists—are a little more sober and modest about it at the present time, and sometimes people ask well, really what is economics about anyway?

Do economists agree on anything? You get a room full of economists, you have got a lot of opinions. If you look a little deeper, economists agree on a lot of things. They agree on the basic mechanisms that make an economy operate, on the basic operation of markets, and on the basic operation of prices. But they don't agree very much on one very critical area, which happens to be an area of central importance for predictions, business cycles, and for deciding what, if anything, should be done about them. And that is the area of how human beings form expectations about the future.

When we buy a new house, even when we buy a new car, when we make an investment, we are making a bet on the future, and we make it on the basis of an assumption of what that future is going to look like. We take into account certain factors. We ignore others. It is precisely in that area that you find the major differences among schools of economics as far apart as Milton Friedman, with his monetarism or Paul Samuelson with his Keynesianism.

I don't know how you array people. Take any distribution of economists you like, and you will find them mostly disagreeing in this area of expectations. Now, expectations or how people form them is a factual question, not something to sit in a chair and speculate about. We have to go out and see how people actually behave. What we need in that area of economics—what we need to close this gap in our economic knowledge is massive empirical research of a kind that has not been done much in economics. Economists are used to using publically gathered statistics, quantitative statistics, to make very sophisticated analysis.

Econometrics has burgeoned in the last 40 years, but economics has not been accustomed to going out acquiring techniques of finding how human beings form their expectations about the future. Economics is going to have to learn those techniques, and economics is going to have to be given the opportunity to make those kinds of empirical studies using field and laboratory methods to an extent that they have not been used in the past in that field.

I think we might well compare this part of economics, macroeconomics, that is interested in policy, might well compare it with meteorology. Meteorologists make predictions, too. Unfortunately they don't make them accurately enough for us to know when to carry an umbrella, and as a result of that, we are sometimes unhappy about them. That doesn't mean meteorologists don't know anything or economists don't know anything. It means we have to go a good deal further to get the knowledge that is important for our practical purposes.

That leads us from expectations into the general area of decisionmaking. There has been a powerful advance on the formal mathematical side in our understanding of the decisionmaking process, something which I don't try to explain in detail, called the theory of subjective expected utilities. As a matter of fact, Professor Luce would be the right man to explain it if anybody is going to explain it—at any rate, a very formidable mathematical theory about what would it mean to make decisions rationally.

What would be the rational way of taking into account the consequences of different alternatives and the probabilities attached to them in the light of one's preferences, and in the light of one's utility? That theory has finally honed within the last 30 or 40 years, and has proven to be very important as a descriptive theory; that is, many of the management science and operations research tools are based on the notion of maximization of expected utility, and those tools are used today in very practical ways to plan the blending of oil in oil refineries, scheduling factories, planning tree harvests, mixing cattle feeds. All of those practical activities have been fitted from OR tools which make use of these modern decisions theories.

These theories have done a great deal for us in conceptualizing the choice process. What they haven't done yet is to take into account of how that notion of rationality is modified when we take into account the limits on human thinking and when we take into account the fact we human beings have very limited capacity to handle information, to make computations, compared with the complexity of the world that we are dealing with. So the next stage and the stage that is already beginning in decision theory is to build a descriptive theory that takes into account actual human limitations in what might broadly be called computational capacity, human limitations on how much we can know and how deeply we can think.

Very fine work done, for example, by a psychologist named Conneman Tersky, has shown important systematic ways in which actual human decisionmaking departs in the rational model and begin to give us insights into why that departure takes place.

Empirical studies, for example, of flood insurance—who buys flood insurance and why? Well, you can work out a nice analysis of who ought to be buying flood insurance in terms of the probability they are being flooded. The facts have nothing to do with that. The facts have to do with how many friends and neighbors one knows who have been flooded, or whether one has been flooded, and has little to do with the odds of the matter as far as purchase of the insurance is concerned.

These are the new facts that we have to learn about how human beings actually do decisionmaking if we are to understand the whole process, and in fact improve it.

Finally, I mentioned that there is a great swelling up of interests in something today called cognitive science. That is a name that is not more than about 5 years old, and has to do with the study of what are called human higher mental processes, problem solving, and concept formation, use of language and so on.

Why has this come about in the last few years? Why is there sudden interest in cognitive sciences? What is a good scientific problem? It has to have three characteristics. First, it has to contribute to basic knowledge. Why study it if you aren't going to contribute to basic knowledge? Second, it has to have ultimate promise of usefulness. Society isn't going to support research unless that research is going to pay off or promise to pay off the society. But, third, it has to be doable. Physical scientists don't go around doing physical research on gravity shields. A gravity shield would be a great thing to have. You go off and research on something more important, that you have a chance of getting, and, similarly, a new field like cognitive science grows up when we see new opportunities to gain knowledge. It is very important knowledge. It has very important practical applications.

And basically, what has created this new cognitive science—I am grossly oversimplifying—is the computer. The computer first gave us new ideas how we can conceptualize human thinking; and gave us second, a very powerful tool which has proved effective in modeling many aspects of human thought. So today, for example, we can write a computer program which simulates what a doctor does when he makes a medical diagnosis and can do it at a very good quality level.

As far as I can determine from my inquiries, the only reason those techniques aren't used in practice today is nobody has figured out who would be liable if a misdiagnosis is made by the computer. We have models today of how kids learn algebra. We have models of how people go about writing, go about composing some piece of paper they are going to write, like, a piece of testimony, let's say. These fields are attracting scientists. These questions are attracting scientists from psychology, from linguistics, from artificial intelligence, from the other social sciences.

As we get this deeper knowledge of human beings, we have new aspirations for possible applications to human affairs. I mentioned medical diagnosis already. One that I find most exciting is learning theory. We now are learning how to—we are learning how to diagnose children's errors when they are acquiring a new skill like skills in arithmetic. We are beginning to understand how to build computer-aided tutoring systems that are not drill and practice, but make use of the computer's ability to understand what is going on and to respond flexibly to the student. We are learning a lot of things about what makes a good or bad text book in terms of what needs to be there so that a skill can be acquired from it. So, we can anticipate a payoff for the educational system.

I have offered three examples here of areas of behavioral and social sciences that have already reached a significant level of development in their ability to deal with complex, human and social phenomena which are capable of moving ahead today at a pace that I think will be only limited by human and financial resources, and I think human resources are there and ready to be marshailled.

I mentioned three criteria for judging the merit of research projects, and these three areas, I think, surely—and they are only examples—surely score high on all three of those criteria. They refer to the theoretical cores of important disciplines. Their importance to our human affairs is great and massive, and in each of these areas we now have powerful toolings of research, or are acquiring them that let the people in the fields know what steps are the next steps to take ahead.

After my colleagues have had a chance to comment, I would be very happy to answer your questions and comment further on these matters. Thank you.

[The prepared statement of Dr. Simon follows:]

Statement Before

Committee on Science and Technology
U. S. House of Representatives

Tuesday, September 17, 1985

Herbert A. Simon

Richard King Mellon University Professor
of Computer Science and Psychology
Carnegie-Mellon University
Pittsburgh, PA 15213

Mr. Chairman and Members of the Committee on Science and Technology:

My name is Herbert Simon. I am a Professor of Computer Science and Psychology at Carnegie-Mellon University. A few years ago, I chaired a committee for the National Academy of Sciences that reviewed the Social and Behavioral Science programs in the National Science Foundation, and on several previous occasions I have participated in the work of committees of the Academy's Committee on Science and Public Policy that examined the problems and prospects of the social and behavioral sciences in the United States. My own research in recent years lies in the area of cognitive psychology and the neighboring area of artificial intelligence. At an earlier time I worked in economics and organization theory.

Professors Luce, Smelser, and I have agreed on a rough division of labor, so that, collectively, we can comment on a wide span of the social sciences, avoiding excessive overlap. Beyond that, the views of each of us is his own. In my own remarks, I will pay particular attention to research in economics on forecasting and decision making generally, to the work on individual decision making that is going on mainly in departments of psychology and schools of business, and to research in the newly christened field of cognitive science, a highly interdisciplinary enterprise.

The Social and Behavioral Sciences Today

The social and behavioral sciences are no newcomers. The same fact-seeking and rational spirit that spawned the physical and biological sciences gave birth also to the social sciences. The first mortality tables, and actuarial tables based on them, are 300 years old. Adam Smith wrote more than 200 years ago, and mathematical economics (Cournot) is 150 years old. Psychologists have been conducting systematic laboratory experiments on memory, learning, and perception for 100 years. The basic ideas of psychological testing were laid down at the beginning of this century. We have been gathering systematic census and other survey data about ourselves since the Republic began, and modern public opinion polling methods have developed over a half century.

Just mentioning these few examples illustrates both the longevity of these fields and what an essential role social science information plays in our everyday lives. We cannot pick up a newspaper without being confronted with facts about the age distribution of our population, trends in its diseases and mortality, the level and distribution of its income -- I could go on indefinitely. These and other social science data inform us about the world in which we live, shape our views and social attitudes, alert us to problems, affect the ways we raise our children, manage our cities, regulate the economy.

The true impact of social science knowledge on our society is sometimes hidden by the decentralized ways in which it is communicated. The products of physics research are brought to application largely through the community of physicists and engineers; the products of biological research through the biologists and physicians. But in social science, people are their own engineers and physicians. This fact creates some special problems for the social sciences, and especially the applied social sciences, but it also underlines the critical importance of social and behavioral knowledge for our society. Perhaps the most important role of the social sciences, among their many roles, is to provide this basic fund of knowledge about ourselves and our institutions -- a foundation of reality for the thinking and decision making of legislators, managers, both governmental and corporate, and all of us as citizens, householders, and employees.

Application of science must rest on basic knowledge. What can we say about the growth of basic knowledge in the social sciences? Perhaps the most reliable measure of the health of an area of basic science is the excitement it engenders -- in those who are engaged in the research, and especially in the bright new graduate students who are attracted to it by the prospect of rich lodes of discoveries to be mined. My own research career now spans almost exactly a half century. During most of that time, high-energy physics has surely been one of the "hot spots" of scientific excitement. Molecular biology has been another. A third has been astrophysics. Another has been mathematical economics and formal decision theory. And for the past thirty years cognitive science has provided a bright new focus.

Limiting myself now to the social and behavioral sciences, I have seen remarkable new discoveries made during my working lifetime -- not just a few, but a whole cumulative development that has transformed these fields and continues to transform them today. To see how dramatic the change has been, we need only compare our knowledge of human individual and social behavior today with the knowledge that was summarized fifty years ago in the *Recent Social Trends* report commissioned by President Herbert Hoover, and published in 1933. We have important new kinds of data, much more powerful mathematical and statistical tools for analyzing them, and theories that are both more accurate and conceptually richer than those of our fathers.

But I think that in inviting me here, you expected more than my general impression -- though it is a very strong one -- that the social and behavioral sciences are going through a period of unprecedently rapid advance. Let me turn to some concrete "for-instances" that will illustrate the kinds of knowledge we have been gaining, the prospects for continuing substantial progress, and the past and prospective applications of this knowledge. My examples are selected from topics on which I have some personal knowledge, and in no sense give a picture of the whole domain.

Understanding the Economy

As we shifted from the relatively smooth economic sailing of the Fifties and Sixties into the problems and perplexities of the Seventies and Eighties, economics -- especially macroeconomics, the study of the whole economy -- has lost face. Problems we thought we had solved or could control return in new vigor. The layman is confronted with all varieties of conflicting advice on economic policy offered by the experts. Is there anything economists can agree about? Is there anything they really know about business cycles and their control?

Indeed there is. Our theories about how economies operate are far more sophisticated and valid than they were in the 30's, and the domain of agreement among economists -- even those who we think are spread far apart on the policy spectrum -- is very large. In fact, a major consequence of the advance of theoretical economics and econometrics has been to spotlight the precise small areas of disagreement that remain, and to show why these areas are crucial to economic forecasting and economic policy. If the problem has not been solved to everyone's satisfaction, it has at least been clearly identified -- an important step towards solution.

The biggest gap in our knowledge of macroeconomics today is in not knowing how people in our society form expectations about the future. What factors do people (consumers, businessmen) take into account when they make purchases, when they change jobs or migrate to a new city? What information do they use in making their guesses about future trends in these factors? Are their models of the world stable, or do they change with their experience of events? How do expectations influence businessmen's investment decisions? The major differences among "Keynesians," "monetarists," "rational expectationists," "supply siders," and the other brands of economists who seek to advise us stem from differences in their assumptions about how people form expectations.

The development of economic theory during the past half century (and especially the theory of decision making, of which I will say more presently), and the great advances in statistical and econometric techniques for analyzing data during this same period have produced today's area of consensus and focused our attention on the central role of expectations in economic affairs. Since the ignorance that remains is mainly an ignorance about *facts* (about how people behave), we may expect that important new progress can be made by ferreting out these facts. This is not simply a matter of making better analyses of statistical data that already exist, although that will help; rather, it is a matter of developing new data-gathering methods, now very little known in economics, for discovering how people form expectations. Here economics can borrow and learn from sociology and social psychology, which have extensive experience in gathering information about human behavior in the laboratory and the field. And we can see beginnings in this direction -- for example, the new interest in laboratory experiments in economics.

I do not mean to imply that theory-construction in economics is going to languish in the future. On the contrary, better facts will call for improved theories, and those theories, in turn, will be used to tune the complex computer models that governments and business firms use to make their economic forecasts. And the rapidly growing interdependence of national economies means that we will need to apply increased research efforts to constructing adequate computer models of the international economy.

There are some close analogies between the research problems of macroeconomics and meteorology. Neither field today produces predictions of a quality that satisfies us. Both fields rely on computer modeling that taxes the largest and fastest computers. In both fields, there are missing pieces of theory and of data that fix the limits of the accuracy currently achievable. And both fields can expect a steady advance toward accuracy of understanding and prediction if resources are made available and used to address these problems.

Theories of Decision Making

At the core of many social and behavioral phenomena lies the process of decision making: of setting goals, finding or designing suitable courses of action, and choosing among alternative actions. Our previous example from economics hinged essentially on the formation of expectations in the decision-making process. One of the major accomplishments of the social sciences in the past half century has been to construct a carefully crafted formal theory of what it means to make decisions "rationally." The theory asserts that the rational decision maker chooses so as to *maximize his subjective expected utility*, and it is therefore known as SEU theory.

The key assumptions of SEU theory are that the decision maker has consistent preferences among all possible futures (a "utility function"), estimates the probabilities of events in a consistent way (expectations take the form of estimates of probabilities), and opts for the best odds (chooses to maximize expected utility). The theory can be interpreted either as an intended *description* of human choice behavior or as a *prescription* of how people ought rationally to make their choices. As a prescription, it has found wide use in the tools of operations research (linear programming, dynamic programming, queuing theory) that are employed in modern industrial management. These tools are used not only in high-tech industries (e.g., for the blending of oil in refineries), but also in forest management and in the mixing of cattle feeds. Of course, before the SEU apparatus can be applied to real problems, the problems have to be redescribed in a highly simplified and approximate form. Nevertheless, the formalism has shown great practical value.

The SEU theory has great value, also, for conceptualizing the choice process, but as a description of human behavior, it falls far short of telling the truth, the whole truth, and nothing but the truth. As a product of an increasing research effort, especially by psychologists, to discover how people actually make choices, the inaccuracies and insufficiences of SEU theory become increasingly clear. For example, the behavior of people in preparing for rare, but potentially very costly, events (e.g., buying flood insurance) cannot be explained even approximately by the SEU model. Today, the SEU theory, interpreted as a descriptive theory, serves largely as a framework on which the deviations of actual behavior from the model can be hung.

Since SEU theory lies at the core of modern economics (micro as well as macro) and statistical decision theory, finding appropriate modifications in it stands high on the social science agenda. The problems with the theory do not lie in its logic -- it is, in fact, a structure of the highest sophistication and mathematical rigor to which some of the best mathematicians, mathematical economists, and mathematical statisticians of our century have contributed. The central problem is that the theory does not pay proper attention to the limits of human beings -- of ourselves. It does not take into account how limited and inaccurate is the information we use in making decisions; how few of the potential options of choice we can discover or invent; and how limited is our ability to draw inferences and conclusions from the information that we do possess.

In short, the difficulty that has been found in decision theory can be traced to human *bounded rationality*: to the limits of human information-gathering and thinking capacities in relation to the complex world we live in. Computers may help stretch the limits, but even the biggest existing and prospective computers are puny in comparison with the world they are asked to analyse.

Simply recognizing human rationality as "bounded" does not provide a specific theory, with predictive power, that can replace the SEU theory in its present uses. The new theory will undoubtedly be less "elegant" (but far truer to the facts) than the one it replaces. As in the case of a theory of expectations (which is one part of it), it will call for very extensive laboratory and field research, examining a wide range of human decision-making situations under a wide range of conditions.

Today, there is a considerable interdisciplinary stirring of research scientists in virtually all the social sciences, but especially psychology and economics, who see the need for building a more elaborate and realistic theory of decision making, and who are beginning to carry out the vast amount of experimentation and field research that will be required to reach this goal. Theorists, too, especially in the domain known as "the new institutional economics" are now busy constructing explanations of the behavior of business organizations and of legal institutions that take account of more of the limitations on human rationality than have been considered in the past.

I hope that my examples do not give the impression that social science takes one step backward before it takes a new step forward. That would not gain much ground. Relativity theory and quantum mechanics did not *refute* classical physics, but extended and enlarged it. The classical theory still remains a highly useful approximation, applicable to a wide range of situations. Similarly, an empirically based theory of expectations will not displace classical economics, but will build upon it and remove some of its inadequacies. The developments now going on in decision theory are cumulative in the same way. As in all sciences, it is the current theory that reveals its own inadequacies as new factual evidence accumulates and prepares the way for its successor. The path of science may be spiral, with many twists and turns as we move forward; it is not circular. My final example: the rapidly developing field of cognitive science will illustrate the same point.

Cognitive Science

There are three basic criteria for selecting a good scientific research question. If the research is successful, (1) will it contribute to basic scientific knowledge and/or (2) to the solution of human problems; and (3) is it judged to be do-able -- do we know how to take at least a first step towards its solution. Moreover, the first two criteria are closely interwoven. Any fundamental progress we make toward understanding economic, or political, or psychological mechanisms is a long step toward understanding the human problems in which those mechanisms are involved.

We think less often about the third criterion than the others, although it probably deters us from doing research on gravity shields or perpetual motion machines. Sometimes a problem is so important to Mankind (for example, the problem of peace) that it merits a large research effort even if our tools are inadequate and the prospects of rapid advances in knowledge are bleak. But my final example today illustrates the much happier situation where new research instruments transformed steady but slow progress into a major and sustained speedy advance.

Early in its history, experimental psychology was "burned" by an incautious use of

BEST COPY AVAILABLE

introspective methods to gather data that could not be verified publicly and objectively. Out of this experience grew the strict methodological rules of Behaviorism, which, with the instruments of experimentation then available, confined psychology to research on simple phenomena that could be studied objectively and wholly from the exterior: reactions to simple stimuli, learning rote verbal lists, and the like. Though experimenters were ingenious, many complex forms of human behavior -- problem solving, concept formation, use of language, complex learning -- seemed to be beyond the pale of "scientific" psychology. Only the Gestalt psychologists, mostly European, continued working in these difficult areas, with a considerable sacrifice of rigor.

The period of the Second World War produced powerful new instruments for the experimental laboratory and for theory building. There were new electronic instruments for recording reaction times with great accuracy, and instruments for presenting stimuli with precision. The tape recorder allowed verbal behavior to be analysed in detail and at leisure; the eye-movement camera tracked subjects' focus of attention.

Above all, the modern computer assisted in all of these tasks (especially stimulus display), and also provided a new theoretical possibility. It suggested the idea of modeling thought processes, not by translating them into mathematical form, but by using the symbol-manipulating capabilities of the computer to simulate the symbol-manipulating processes of the human problem solver or learner.

The new electronic devices, including the computer, provided not only new instruments but also new ways of conceptualizing human thinking. The massive change they have wrought in cognitive psychology is now called "the information processing revolution." The effects are visible in every psychological journal. First, there is a much greater emphasis than before on research on complex tasks: tasks at the level of professional work or school learning. Psychologists today are willing and able to study the psychological processes of a medical diagnostician or of a student learning algebra or attempting to write an essay.

Second, there is considerable agreement on concepts and mechanisms for interpreting experimental results. Theories, some in mathematical form, some expressed as computer simulation programs, are far more precise than earlier theories, and can be tested over wider task domains.

Third, there is a great intermingling of disciplines. Experimental psychologists, linguists, researchers in artificial intelligence, philosophers, and others have entered into the common activity, and communicate with each other through such journals as *Cognitive Science*, *Cognitive Psychology*, and *Artificial Intelligence*.

Finally, the progress in cognitive psychology has raised new aspirations for the application of psychology to important problems in education, business and professional decision making, and man-machine interaction.

I mentioned medical diagnosis earlier. Let me use it as an example. There have been several major studies of how physicians process information while making a medical diagnosis. The processes that have been revealed, and the way they are organized, turn out to have much in common with the processes and organization of expert behavior in other domains that have been studied (for example, designing motors, playing chess, solving physics problems).

At the same time, several artificial intelligence programs have been written that are capable of making diagnoses in particular domains of medicine (internal medicine, microbial diseases) at a good clinical level of quality. These systems have not yet been used much in practice (partly because questions of legal liability are unanswered), but they already show considerable promise for use in the teaching of medicine.

Some of the most exciting developments in cognitive science relate to what we are learning about language, and I think Professor Luce intends to say something about those. Equally exciting things are happening in the domain of learning theory. Cognitive theory is now beginning to provide principles and procedures for diagnosing children's errors, when the are learning new skills, for building computer-aided tutoring systems, and even for writing textbooks. Schools of education are beginning to take note of these developments, and I think we can look ahead to an exciting and productive decade -- or several decades -- of educational research. In fact, it is already time to begin thinking about the kinds of institutional flexibility we will need to absorb and exploit these new developments in our schools.

Conclusion

I have offered three examples of areas of the behavioral and social sciences that have already reached a significant level of capacity for dealing with complex human and social phenomena. These areas are capable of moving ahead today at a pace that will be limited only by human and financial resources (and the excitement of the research is attracting the high-quality human resources they need). Earlier, I mentioned three criteria for judging the merit of research projects. The examples I have discussed -- and I emphasize that they are only examples -- surely score high on all three criteria. They are building the theoretical cores of important disciplines. Their importance to our human affairs is evident and massive. And in each of these areas, we have powerful tools of research, or are acquiring them, that will allow us to take the next steps ahead.

I shall be happy to answer your questions, or to comment further on any of these matters that are of interest to you.

Mr. WALGREN. Thank you very much for that presentation, Doctor. Let's turn to Dr. Luce.

[A biographical sketch of Dr. Luce follows:]

DR. R. DUNCAN LUCE

R. Duncan Luce, Victor S. Thomas Professor of Psychology, William James Hall 930, Harvard University, Cambridge, MA 02140, (617) 495-3815.

Education: BS, 1945, in Aeronautical Engineering and PhD, 1950, in Mathematics from the Massachusetts Institute of Technology.

Military Service: USNR, 1942-46, Ensign.

Teaching Positions: Columbia University, University of Pennsylvania, University of California at Irvine, and Harvard University.

Other Academic Positions: Staff Member, Research Laboratory of Electronics, M.I.T.; Fellow, Center for Advanced Study in the Behavioral and Social Sciences, Stanford, CA; Visiting Professor, Catholic University, Rio de Janeiro; Visiting Professor, Institute for Advanced Study, Princeton, NJ.

Professional Societies: American Association for the Advancement of Science, Fellow; American Mathematical Society; American Psychological Association, Fellow; Mathematical Association of America; Psychometric Society (past president); Psychonomic Society; Society for Mathematical Psychology (past president).

Honors: American Academy of Arts and Sciences; Distinguished Scientific Contributions Award of the American Psychological Association; Guggenheim Fellow; National Academy of Sciences, member, past chairman of the Section of Psychology, and chairman of the Class of Behavioral and Social Sciences; Senior Post-doctoral Fellow, National Science Foundation; Phi Beta Kappa; Society of Experimental Psychologists; Sigma Xi; Tau Beta Pi.

National Committee: National Research Council: Assembly of Behavioral and Social Sciences, past Chairman; Committee on US-USSR Seminars in Experimental Psychology, Chairman; Committee on Basic Research in the Behavioral and Social Sciences, Co-chairman; Report Review Committee. National Science Foundation: Advisory Committee to Division of Information Science.

Books: (with H. Raiffa) *Games and Decisions*, 1957; *Individual Choice Behavior*, 1959; (with D.H. Krantz, P. Suppes, & A. Tversky) *Foundations of Measurement*, I, 1971; *Response Times*, in press.

Edited Books: (with A.W. Tucker) *Contributions to the Theory of Games*, 1959; (with R.R. Bush & E. Galanter) *Handbook of Mathematical Psychology* (3 vols) and *Readings in Mathematical Psychology* (2 vols), 1963, 1965; *Developments in Mathematical Psychology*, 1960; (with R.C. Atkinson, D.H. Krantz, & P. Suppes) *Contemporary Developments in Mathematical Psychology* (2 vols), 1974; (with R.C. Atkinson, R.J. Herrnstein, & G. Lindzey) *Handbook of Experimental Psychology*, in press.

Articles: over 100 in psychological, mathematical, and social science journals.

STATEMENT OF DR. R. DUNCAN LUCE, VICTOR S. THOMAS PROFESSOR OF PSYCHOLOGY, DEPARTMENT OF PSYCHOLOGY AND SOCIAL RELATIONS, HARVARD UNIVERSITY, CAMBRIDGE, MA

Dr. LUCE. My name is Duncan Luce. I am a professor of psychology at Harvard University. My areas of research have to do with mathematical models in psychology and in social sciences more generally. Currently, I am cochairing with Professor Smelser, a committee office of the National Academy of Sciences and National Research Council on basic research in the behavioral and social sciences.

We are conducting for the National Science Foundation and several other agencies, a study that is aimed at developing priorities during the next 10 years for the incremental funding beyond current levels in the behavioral and social sciences. I want to take a few minutes and outline how that study is being carried out, and then cite a few examples of interesting areas of research that are underway.

I list six in the paper. I will deal with only three in the oral testimony. I will take every other one. And then I will talk about some of the conclusions as to the type of new funding that appear to be developing from this study. I should emphasize that study is midstream and that these are my personal impressions, and by no means are they the committee's conclusions.

We have not yet gotten to that point. The study, again, was sending out a list of requests to some 2,500 behavioral and social scientists and to the readers of 150 journals—a request for what they thought was the exciting activity, the most exciting thing they knew about going on. We got about 1,000 suggestions with obviously some overlaps. The committee then wrestled with that massive material and ultimately decided to form 30 subareas in which we would have working groups that would take the terms that were provided, and their own knowledge, and consult with colleagues and generate 20-page reports, which often turned out to be 30 pages. And then the committee is going to use that as its raw material in developing the report.

We have not yet met. We are going to meet in early October to deal with that. So having read those reports, I have some notion of what is in them, and I would like to share some of that with you. I will confine my attention to behavioral aspects, including some social psychology, and leave out cognitive since Professor Simon has dealt with that, and Professor Smelser will deal with more social science topics.

The first one I will take up is perception. One of the characteristics of the work in perception—this is one of the old fields of psychology. Current work today involves a change that is due largely to the computer, the ability that we now have to generate much more complicated stimuli overtime, the ability to handle much more massive data assets, and the ability to analyze, sometimes by simulation, mathematical models of what is going on.

It has opened up enormous possibilities. And one of the examples of a current piece of work that I know of is a professor at Stanford University who has got an idea that is now being tested empirically, and has also generated a patent, because if it is right, it probably has very important practical applications. One thing we can do, and any mammal is able to do apparently, is perceive a scene under a variety of lighting conditions and somehow the color relationships in that scene are relatively invariant from the lighting that happens to be applied. And we somehow are able to separate what are the intrinsic color relationships from the lighting. No one has known how the brain has done this, and what he has is an idea how this is done, which if it is right, will mean that there will be a revolution in the photography and television industry. Because you will be able to photograph scenes under any lighting conditions, decompose the effect of the light source from the scene, and then recompose it with any light source you want to assume, you can take a photograph under, let us say, cloudy conditions and then be able to produce a print that would be as if it had been under, say, bright conditions.

A second example of an active area of research I will be talking about at a much more general level than the specific example is linguistics. Linguistics has gone through several periods of important revolutions. One that I think is most familiar to many people is Chomsky's revolution of about 20 years ago. More recently, there are some new ideas along those notions of Chomsky, but more general, having to do with invariants that exist apparently across all languages—very deep universal invariances that are not biologically determined in the organism. They have a great deal to do with the way we acquire language, independent of which language we are acquiring.

This set of ideas is now at a very exciting stage where developments involving computer modeling, on the one hand, neurological observations on the other hand, and developmental observations of children acquiring language, are all beginning to come together to give us the ability to test out these ideas, which if they turn out to be correct, probably have very far-reaching consequences. For instance, one thing that we would very much like to be able to do is mechanically, through a computer, translate from one language to another. This has shaped us as to how to do that, and it presumably is going to come about when we have a sufficiently deep understanding of the common structures of languages that we are going to be able to carry it out. I will return to the linguistic example because it leads to some of the proposed kinds of research change that I think is going to come out as one of the recommendations of our committee.

A third example comes from social psychology and concerns questions that have been raised about jury behavior. This is clearly a topic of considerable interest to the legal profession.

For instance, the kind of question that has been raised is: Can we in some measure change from the 12-person unanimous jury to smaller juries with rules other than unanimity, and if we do make such changes, what gains and losses will occur? The gains are fairly clear. It will be cheaper. The smaller the jury with a less than rule of unanimity, presumably the decisionmaking process will occur more rapidly, but there is a question of whether the quality of the decisionmaking will be seriously altered.

The difficulty in studying it, as you well know, is that for legal and ethical reasons, one can't study actual juries in action, and the issue has been that you can't really count on using recollections of jurors as to what happened. That is a notoriously unreliable source of information, so that something else had to be evolved. And what has been evolved is the construction of an artificial trial on videotape, but modeled on an actual transcript of actual trials, and going to a jury pool that has been assembled for the regular jury purpose. When the juries of the day have been formed, there is usually a group of people left over that are not called; getting them to participate, then, in this study, to have them listen to this trial, and then proceed in a regular jury room as if they were a jury, but knowing that they are being observed. And again, the availability of modern technology, videotaping and computers has made possible a very detailed study of what goes on in those jury rooms.

I will summarize a few of the results, but this is only a small sampling of the kinds of results that have been found. As you might expect, the deliberations are in fact briefer and less thorough when you are not using the unanimous rule. Moreover, it is found that with the unanimous rule, the most important part of that discussion goes on after the point at which a decision would have been reached.

With a nonunanimous rule, after that majority had been achieved in the unanimous group, a period of discussion takes place in which serious errors are unearthed, both of fact, in the trial, where some of the jurors have misunderstood the testimony, and errors in understanding the legal definitions of verdicts that are possible or the judge's instructions to the jury.

Third, the nonunanimous juries tended more often to either extreme verdicts. For instance, in a murder trial they will tend to either first-degree murder or an acquittal rather than second-degree judgments, which was in the particular example that was used, was the model decision. In fact, legal authorities felt that second-degree murder was the appropriate outcome of the deliberations.

The fourth point is that members of small dissenting factions participated less in the discussion when the rule was not unanimous, and they were very much less satisfied with the outcome of the decisionmaking process. The consequence of that is that one would be generating systematically a set of people who were somewhat cynical about the process that they engaged in.

Now, out of these various studies, of which this is a tiny sample of things that have been brought to our attention, certain suggestions have recurred as to the type of funding that might be carried out if we get some incremental funding on top of the current levels. Let me be clear that all of the people that have been involved are very concerned that we not be misunderstood; that they feel that the current system of relatively small peer reviewed grants is something that is very important and should be left in place. It should not be destroyed.

It has certain rather clear virtues. One is that new ideas can enter into the system very readily when you have this kind of self-generated grant proposal, whereas, if you begin to have a fairly monolithically controlled system, it is often very difficult to get new ideas, particularly ideas from younger people, into the system, because the control tends to be in the hands of older people.

Another feature of the peer review system is that, in a way, it is a marketlike device for setting priorities; that is, things that are not working out, research avenues that have begun to fail, simply get phased out because the peer review rejects the proposals that are tending in that direction—and it does it without a great deal of fuss and bother. That is something that I think should not be underestimated.

With that in mind, let me talk about some things that will deviate substantially from the peer, the small grants peer review structure. One thing that has come out in the linguistic group that is a particular case in point is the need for rather large research centers that are of an interdisciplinary character.

In the case of linguistics, one would want people who are specialists in computational linguistics; people who are specialized in a variety of languages, because if you are looking at these invariances, it is going to be a question of looking at languages of very different sorts; people who are trained in developmental psychology, in the study of language acquisition in children; and neurologists who will study the failures of language in various kinds of medical cases, clinical cases, because if these ideas are correct, one expects a certain commonality of the failures across languages if there really are common denominators to the way the brain is dealing with language. This will require a relatively large center.

Another kind of center that is being proposed by very many people in different areas in the behavioral sciences is a large computer center where one would have an extremely powerful computer. Currently people talk in terms of a Cray, but on the drawing boards are massively parallel computers and many of the modeling problems that we have in the behavioral sciences look as though they will be best handled by a computer that is essentially mimicing the brain in the sense that the brain carries on many many functions in parallel. When you are perceiving a verbal scene, or listening, there is an enormous amount of a parallel activity going on in the brain, and that will, I think, be best modeled when we get to that stage.

In any event, the call is for a dedicated computer center to the social sciences in which you are talking about a pretty sizable budget, and a pretty long time commitment—by sizable budget something on the order of $10 or $15 million to set it up and several million dollars a year operating budget.

Another thing that has come out in a number of studies, and Smelser may give you an example, is the need for long-term longitudinal studies, where you are talking about watching a population of subjects for a period of 10 years, and this requires not only a fair amount of money per year, but it requires a social commitment to funding it over that period of time, because if you start one of those studies and then for 2 or 3 years, midway you suddenly cut the budget, you have effectively lost each of the studies in the process.

A third theme that has come up in a variety of places is the need for summer research institutes where you have something of the order of 10 to 30 researchers getting together for a period of 4 to 6 weeks, and carrying out research—not just exchanging reports of what they have just done, but actually getting research underway, and usually altering research directions through their interaction. These in the past have proved quite successful in the opinion of many of the participants from them, and yet they are not very common today.

Now, on the assumption that there is a consensus for their needs, one can ask the question why they cause a special problem. And I think that there are really three interrelated reasons. The projects tend to be interdisciplinary. They tend to be more expensive than the typical or large research grants, and many of them have a time scale of greater than 3 years. In contrast, in the funding in the behavioral social sciences, there is a tendency for the funding to be organized by disciplinary disciplines, specialty. The grants tend not to be of a modest size, thinking of $75,000 range, and they tend to have a time scale of 1 to 3 years. Now those two sets of requirements are incompatible. What we are proposing just doesn't fit the existing scheme very well. I think one of the things our committee will have to try to do is make proposals about how the funding might be modified in order to accommodate these needs that are felt by quite a wide range of people.

When I started out, I thought some of these like the large center ideas, would be just in a very few places, but they come up in quite a few of the working group reports. I will, at this point, conclude and later be prepared to try to answer questions.

[The prepared statement of Dr. Luce follows:]

Statement of R. Duncan Luce

Victor S. Thomas Professor of Psychology

Harvard University

Cambridge, MA 02138

before

Committee on Science and Technology

U. S House of Representatives

Tuesday, September 17, 1985

Luce

Mr. Chairman and Members of the Committee on Science and Technology

My name is Duncan Luce. I am a Professor of Psychology at Harvard University. A biographical statement is attached. I am currently the co-chairman, with Professor Neil Smelser, of the National Academy of Sciences / National Research Council Committee on Basic Research in the Behavioral and Social Sciences, which is currently conducting, for the National Science Foundation and several other agencies, a study designed to develop priorities over the next ten years for incremental funding beyond current levels in the behavioral and social sciences. Our task is to search for interesting opportunities and innovative ways to accelerate growth of knowledge in these areas.

I will outline briefly the study plan. During the spring of 1984 we sent requests to some 2500 behavioral and social scientists and to readers of 150 key journals asking which topics seem most exciting at present and what institutional changes are needed to facilitate the research. (A copy of the letter of request is attached.) During the fall and winter of '84 the committee wrestled with some 1000 suggested topics and ultimately decided on 30 areas that appeared to encompass a number of promising directions. (A list of these working group areas and their chairpersons is attached.) Let me be clear, this list was not intended to include everything we know to be important, rather it is a selection of areas we have reason to believe hold unusual promise for interesting results in a ten year time span.

The working groups, which were composed of from five to twelve scientists, were asked – indeed, challenged – to prepare a 20-page consensus telling us what they felt to be the most promising new directions in their area and to suggest ways, including significant institutional changes, that would facilitate this work. The groups met once or twice, examined the relevant suggestions we had already received, and of course communicated extensively by phone, mail, and electronic mail during the first half of this year. Their reports are all now in our hands.

Our Committee on Basic Research will spend this fall and winter digesting and debating these reports and related materials among ourselves and with our colleagues. As is obvious from this timetable, neither Professor Smelser nor I can tell you at this time exactly what the committee will finally recommend. However, I can describe to you a small sample of the research that is under way and offer my personal impression of some major thrusts of the proposals that have been made by these groups. My focus will be on certain behavioral and psychological topics -- but excluding cognitive psychology, which I assume will be covered by Professor Simon -- that lie close to policy questions, clinical application, or other practical benefits. In particular, the areas are: perception, psychobiology of learning and memory, linguistics, health and behavior, social psychology, and methodology.

SOME RESEARCH DIRECTIONS

Perception. If asked, what new directions are being taken in sensory psychology?, my reply would be a greatly increased focus on how the organism perceives complex stimuli, often varying in time, in contrast to earlier work on very simple stimuli. These studies are motivated by perfectly ordinary, but exceedingly complex abilities. In audition, how is it that we can single out a voice in a hubbub of background noise? In vision, how is it that we perceive complex objects as unitary and how is it that they seem much the same under enormous changes in lighting? The fact that we, and all mammals, carry out these processes effortlessly -- at least until the mechanism for doing it starts to fail -- does not mean we have any idea how it is done. Note that we cannot do any of these things with a modern computer, despite the fact much profit hinges on the solution to any of them.

I can report that recently a psychologist at Stanford University appears to have a viable theoretical idea about how it is that we separate the light playing on the scene from the inherent color, depth, and texture relations among the objects. Large scale computations are being carried out to test the idea. If he is correct and if, as anticipated, the technology for recording a visual scene digitally can be made sufficiently compact, there could result a revolution in photography. We will need but one type of "film" medium which can be used under any circumstances because the "developing" process will decompose what is inherent to the scene from the lighting that happened to exist at the time of the photograph. Thus, you will be able to "print" the picture with the lighting conditions of your choice, not just the one that happened to obtain when you took the photograph. The "developing" process, which will be carried out on a computer, will be analogous to what the brain does all the time.

Psychobiology of Learning and Memory. Equally taken for granted, at least until it fails, and equally mysterious is memory. We know how computer memories are organized, but there are many reasons to think that this is a poor analogy to human memory. Yet, until quite recently we could find no physiological evidence for how animal and human memories are laid down, which has meant almost total frustration in dealing with deficits in memory arising from trauma or disease or age. That situation seems to be changing. In the past few years, unexpectedly rapid progress has been made in identifying essential memory trace circuits that code, store, and retrieve associative memory in the mammalian brain.

Such work combines, on the one hand, laboratory animal studies that investigate and catalogue the visible behavior patterns corresponding to various types of learning and memory with, on the other hand, various kinds of electrophysiological, physiological, biochemical, and anatomical interventions. Both of these entail laboratories requiring state-of-the-art electronic and optical equipment. But a relatively new aspect of the psychobiology of learning and memory has to do with theoretical or computational modeling of learning and memory circuits in the brain. As memory circuits are defined empirically in the nervous system, it becomes essential to understand quantitatively what these circuits are capable of doing. This can be achieved only by mathematical/computational modeling. There is increasing evidence that more complex phenomena of memory and cognition can emerge as collective properties of neural networks -- properties of large interacting systems of neurons that individually can change their response characteristics.

It is impossible at this juncture to conjecture specifically about the clinical results of a better understanding of how memory is stored and retrieved in the brain. But it is safe to say that effective therapies, whether behavioral or chemical, for memory dysfunction are far more likely to occur as we come better to know how the brain functions.

Linguistics. Closely related to memory research, and yet quite distinct, is the study of language and linguistics. The last few years have seen a shift in linguistics from the earlier emphasis on the rules underlying the structure of the grammars of particular languages to the search and discovery of general principles that underlie the grammars of every (there are, at least, 4000) natural languages. The next decade will almost certainly see a concerted effort to uncover these principles and to demonstrate the likelihood of their universal character by verifying them in large numbers of very different languages. Should this point of view prove correct, then additional evidence for these principles, which in some sense are innate in the human brain, should be exhibited in the ways that children learn languages. Moreover, their neurophysiological basis will receive a great deal of attention. In particular, the breakdown of language seen in aphasics will be examined closely in different language groups to see if the failures correspond to the universal principles. Furthermore, neurological methods that are based on computer-aided, brain-imaging techniques afford a way to localize the region involved.

Once again, no one can make serious promises about the nature and timing of applications of such research, but it is certainly plausible that as our understanding about the nature of language improves and deepens, the opportunities to intervene successfully when there is a failure of language and to improve on teaching both first and second languages can only increase. Indeed, some of the detailed formal models of language learning have guided the development of computer-aided instruction. Furthermore, as our knowledge deepens, the ability to devise computer software that responds to natural language, spoken or written, can only be aided, as will be our ability to develop methods of translating mechanically from one language to another. How fast these applications will come about is unclear, but I doubt there is any way they can come about except through increased understanding of the basic nature of language.

Health and Behavior. The purely medical model of disease is now widely questioned, not only for areas in which choice clearly plays a role, such as addiction, but in more fatalistic matters such as cardiovascular disorders and cancer. Currently there are numerous studies implicating a complex interplay of genetic, behavioral, environmental, and physiological factors underlying many major health problems.

Perhaps the two areas in which behavioral questions are most manifest are in the acquisition and maintenance of addictions, including overeating, smoking, drinking, and running, as well as illegal drugs. This is an area receiving a good deal of attention, especially since it is believed by some to involve a basic motivational mechanism, which in the case of the addict has in some way gone awry in dealing with his or her particular mix of experience, environment, and genetics. Of great interest are those factors that protect some individuals, apparently as much at risk as those who succumb, from the addiction. The nature of the current research is not so much on correlations as on behavioral, brain, and genetic mechanisms that underlie the observed correlations.

Areas in which the behavioral component is less obvious, but recent research strongly implicates it, include, in addition to heart and cancer, the immune system itself and the phenomenon of aging. There is much work on how the central nervous system and through it behavioral and environmental conditions affect the performance of the immune system. Aging, while inevitable, apparently is not as determinate or immutable as was once believed; evidence has been developed that, once again, behavioral factors -- such as control of even apparently minor aspects of one's life -- are powerful determinants of the quality of life of our older citizens and of their degree of dependence on others for health care and maintenance.

These developments, which have not always been welcomed by the medical profession although various signs of change exist, are comparatively new. They appear to be a growing wave, one in which the basic science and the desired social consequences are congruent.

Social Psychology. The legal system has long been interested in what consequences would ensue when one deviates from the 12 person, unanimous jury. Presumably, a smaller jury, say eight, with a six out of eight decision rule, would be cheaper and faster. Would it be less accurate and, if so, how much less accurate; and would it be perceived as less accurate even if it is not? One can of course speculate, and jurists have, but there has been little empirical knowledge to base it on. What goes on in the jury room is simply not subject to examination except, perhaps, by later reconstruction from the memories of the jurors. Such reconstructions are notoriously unreliable.

A social psychologist at Northwestern University has devised a socially and legally acceptable way to bring social psychological methods to bear on the question.

The technique is, first, to place on videotape a re-enactment of a real trial, using professional lawyers and judges. With the permission of the courts, he went to the pool of potential jurors who had arrived for jury duty but were not selected for a trial, and he got them to agree to serve as "jurors" in a simulated trial using the videotape. After watching the trial, they met in regular jury rooms to arrive at a "verdict". Among other things, he manipulated the size and decision rule employed. Their proceedings were, known to them, recorded and videotaped, and then subjected to intense analysis. The findings appear to be reasonably clear cut, and have been of interest to the judicial community. For example, in comparing different decision rules they found: (i) when unanimity is not required the deliberations are far less thorough than when it is required; (ii) a large fraction of the additional discussion under the unanimous rule occurred after a decision would have already been reached under the non-unanimous rule, and serious errors of fact and law were often uncovered and eliminated during that discussion; (iii) the non-unanimous juries tended more often to extreme verdicts (e.g., first rather than second degree judgments of murder) than those reached by unanimous ones; and (iv) members of small dissenting factions participated less in the discussion and were less well satisfied by the verdict than were the minority jurors under a unanimity rule.

Methodology There is no doubt that the behavioral and social sciences have had difficulty in achieving systemization and predictive power -- as, of course, is also true of meteorology, geology, evolutionary biology, etc. Three reasons are: (i) the often very small sample sizes we can obtain either because of expense or because of difficulty in getting phenomena to repeat, (ii) the inability in many situations to carry out simple experiments either for practical or ethical reasons and the need to deal either with only partially controlled interventions or naturally occurring observations, and (iii) the difficulty we have in figuring out how to measure or otherwise represent abstractly various attributes, because the most common measurement operations adequate for physics -- especially addition -- do not apply very well to many behavioral attributes.

These methodological issues are not restricted to particular content areas and, indeed, may be applicable to sciences other than the behavioral and social ones. Statistical developments, which continue with great vigor, are widely employed throughout the sciences, especially when their sample sizes grow small, as they have in a number of recent physical experiments. Much of the thrust for these statistical developments, however, has arisen from the social sciences. The methods of multidimensional scaling, which arose primarily in psychology as a way to organize complex bodies of data into geometrical representations, have found application throughout the behavioral and social sciences, operations research, biology, and the physical sciences. The last few years have seen new developments in our understanding of the conditions under which numbers can be used to represent scientific attributes and laws. These results, which generalize the earlier accounts of why numbers have proved so uncannily effective in the physical sciences, show how it is possible to retain some of the desirable features of physical measurement without having to invoke the operation of addition.

INNOVATIONS IN RESEARCH SUPPORT

I shall turn, now, from these relatively specific topics to suggestions for innovations. But, first, let me reiterate that this is my own reflection on some of the things suggested by the working groups and does not represent deliberations of the Committee on Basic Research.

I feel strongly -- and many colleagues concur -- that the existing arrangements for competitive peer review of individual investigator-proposed research grants of modest size ($25-75,000) and limited duration (one to three years) should not be jeopardized. Our not very monolothic sciences are best fostered and monitored by a system of plural and fairly frequent, disinterested evaluation of actual research achieved and new research freely proposed. Not only is this a way to encourage the best basic research ideas and people around the country, but it is also a way to phase out smoothly programs that either turn out to be unsuccessful or are past their prime. Further, it is a system that can respond. quickly to unanticipated, new ideas. We must recognize the inherent and well demonstrated impossibility of accurately anticipating scientific progress. Who at the end of World War II anticipated the flowering of solid state physics? The peer-review system of research grants is a market-like method of setting scientific priorities.

To attempt to dictate administratively the specific directions and timing of most basic research is not likely to be beneficial compared to the core system now in operation. By and large, scientific progress is made by those who see ways to ask and answer questions of nature. These opportunities for progress do not always -- or even often -- coincide in time with the questions to which we should like answers for various practical or social reasons. To attempt to dictate a specific direction and timing to basic research, from whatever source -- political, administrative, or gray-haired scientist -- is not likely to be beneficial.

This open, competitive, basic core is to be carefully contrasted with applied projects of a highly directed nature when there exists a good empirical and theoretical base of knowledge and one sees clearly the direction of the application. The Manhattan Project was clearly appropriate given the physical knowledge available in 1942. Directed research on cancer has not yet led to a cure for cancer, although the wealth of funds made available has led to remarkable advances in basic biogenetic research, which may eventually make a cure possible. To return to the behavioral and social sciences, I recall the directed attempt, some thirty years ago, to solve the problem of translating written material from one language to another. It was largely inspired by an obvious need and by engineers and some behavioral scientists who thought the problem to be basically a simple computational one. It failed then primarily because we did not then have sufficient understanding of the structure of languages. We still may not, but we are a lot closer and we appreciate more clearly why the earlier attempt failed.

Equally a directed attempt to achieve a really good hearing aid at that time would have failed for at least two reasons: insufficient knowledge of hearing and inadequate electronics. Today we are in far better shape on both counts, and there are better hearing aids that take into account psychophysical results about hearing. But there won't be really superb ones until we understand more fully than we do now how the brain separates out one particular sound pattern -- speech or a particular musical instrument -- from the background of other sounds. The gradual failure of that unconscious mental function bedevils a very large fraction of males over 55. I speak with feeling. Nevertheless, it is extremely doubtful that diverting substantial sums of money directly toward that problem will solve it, because we do not yet understand enough about how the brain does it. I am certain, however, that continuing basic research in audition will ultimately crack that puzzle, and when it does, the application to hearing aids or other therapies will follow quickly.

For the reasons just outlined, the scientific community is very apprehensive about changes to existing grant procedures. With that in mind, let me turn to some of the suggestions that do lie outside the usual grant structure. They arise because research on a number of problems, currently exciting to many scientists, cannot be carried out unless some special provisions are made to allow their support. Some examples:

(1) There is a felt need for a variety of multidisciplinary, multi-investigator

research centers. Perhaps the clearest example is for a computational center or centers dedicated to work in the behavioral and social sciences and built around a supercomputer or, in the next few years, around some form of massively parallel computer. Such a center (or perhaps electronic network of computers) would have to provide the working scientist with the technical expertise to make the computations possible. The existing centers are all focused on engineering and physical problems, and it is difficult for behavioral and social scientists to gain access to them or to have assistance from technical people with relevant experience in their classes of models.

It may come as a surprise that such computations are needed by behavioral and social scientists, but there is little difference in computational complexity between weather forecasting and economic forecasting or between simulating the flight of an aircraft and understanding the processing that takes place in the human brain when we perceive a visual scene. Detailed models of what is happening simply are not feasible except with such powerful computational aids.

The problem here is that the funds involved far exceed those typically granted within a division of NSF, for example. Something on the order of $10-15 million would be needed to set up such a national, multidisciplinary center and it would require an operating budget of a million or two per year.

Another example of an interdisciplinary center arises in language studies where major progress seems possible by combining four areas of expertise: linguistics, developmental psychology, neural studies, and computational modeling. Individuals having serious skills in two of these areas are rare, and more than two non-existent. There is a need for centers where sizable groups of people with such skills can be brought together for long periods of time and where others can be trained in these interdisciplinary activities. Within the currently available structures it is exceeding difficult not only for the research to be conducted, but virtually impossible for a person to receive adequate training in the interplay of these specialties. A careful examination of these needs is in order.

(2) There is an unambiguous need in some areas for relatively long-term longitudinal studies, especially when we want to understand some process of development such as the child acquiring cognitive skills or the nature of criminal careers. Cross sectional time samples and interviewing simply cannot give us the information we need to know to develop reasonable educational and social policies. Here the problem is usually three fold: the work involves a number of people, often from different disciplines, some of whom will leave and others join the project during its life time which may be something on the order of 20 years; the funding must be maintained over that period and must be insulated as much as possible from politics; and the funding is likely to be large even on an annual basis. If such a study, on careful scrutiny, seems the only way to gain the understanding we need and the problem seems important, it is then essential that we devise ways to commit our society to the long-term, stable funding that is needed.

(3) A recurrent theme is the need for summer research institutes. Let me make clear what is meant. These involve from 10 to 30 researchers, varying in experience from advanced graduate students to senior professors, who spend from four to six weeks together focusing on a set of closely related, usually theoretical research problems. Experience suggests that when a field is small -- and many of the specialized subfields of the social sciences are small -- and its top researchers are scattered geographically, this mechanism accelerates and refines research in a way that cannot be otherwise achieved. There were a number of these institutes conducted on a regular basis in the 1950s and '60s, and many of our working group colleagues who participated in them feel they were enormously successful. Oddly,

hardly any occur now. The reason, however, is really simple. At that time a sizable block of money was set aside for the purpose and ideas for institutes were actively sought and their preparation facilitated. Today, such proposals, self originated, must compete as single, isolated efforts against a large number of direct research-grant proposals. As a result, few such proposals are submitted, despite the fact that many researchers feel that such institutes can sometimes be a very effective way to advance research.

Let us assume there is consensus on these three felt needs: specialized centers, long-term longitudinal studies, and summer research institutes. Then, why are they a problem that requires special attention? I think basically there are three interrelated reasons: these projects tend to be interdisciplinary, they tend to be far more expensive than the typical or even large research grant; and many tend to have a time scale greater than three years. In the behavioral and social science area our funding agencies, in contrast, tend to be organized along disciplinary specialties; they can only afford modest-size grants; and they tend to have a time scale of a few years, due in large measure to the up-and-down character of the funding cycle.

I believe that we are in need of mechanisms – still of a peer-reviewed nature – to accommodate to these needs. I expect that our committee will document clearly these or similar types of needs, giving quite specific examples of what is proposed and suggesting that Congress and the funding agencies consider how best to accommodate such important developments. The challenge is to create mechanisms that are equitable and afford efficient intellectual evaluation of these large or unusual proposals, without destroying the essential base of individual investigator grants.

I shall be happy to attempt to answer any questions you wish to pose.

NATIONAL RESEARCH COUNCIL
COMMISSION ON BEHAVIORAL AND SOCIAL SCIENCES AND EDUCATION
2101 Constitution Avenue Washington, D.C. 20418

COMMITTEE ON BASIC RESEARCH IN
THE BEHAVIORAL AND SOCIAL SCIENCES

(202) 334-3017

R. DUNCAN LUCE, *Co-chair*
Harvard University

NEIL J. SMELSER, *Co-chair*
University of California, Berkeley

MEINOLF DIERKES
European Commission on the Social Sciences

JOHN A. FEREJOHN
Stanford University

LAWRENCE M. FRIEDMAN
Stanford University

VICTORIA A. FROMKIN
University of California, Los Angeles

ROCHEL S. GELMAN
University of Pennsylvania

LEO A. GOODMAN
University of Chicago

JAMES G. GREENO
University of Pittsburgh

EUGENE A. HAMMEL
University of California, Berkeley

LEONID HURWICZ
University of Minnesota

EDWARD E. JONES
Princeton University

GARDNER LINDZEY
Center for Advanced Study in the Behavioral Sciences

DANIEL L. McFADDEN
Massachusetts Institute of Technology

JAMES L. McGAUGH
University of California, Irvine

JAMES N. MORGAN
University of Michigan

RICHARD L. MORRILL
University of Washington

SHERRY B. ORTNER
University of Michigan

KENNETH PREWITT
Social Science Research Council

BARBARA G. ROSENKRANTZ
Harvard University

NANCY B. TUMA
Stanford University

ALLAN R. WAGNER
Yale University

DEAN R. GERSTEIN
Study Director

June 25, 1984

Dear :

We write to you and other distinguished colleagues to enlist your help in a challenging task: a ten-year outlook on research opportunities in the behavioral and social sciences.

The Committee on Basic Research in the Behavioral and Social Sciences is charged with identifying potentially productive and exciting research problem areas that could benefit from significant new support. Our final report to the National Science Foundation, Russell Sage Foundation, and other sponsoring organizations will recommend new research resources, facilities, and program initiatives designed to advance fundamental knowledge in the coming decade. An overview of the Committee and the outlook project is enclosed.

We are asking you to identify important topics--research problem areas or resource needs--for focused attention and to name several persons (senior or junior, American or foreign) whom you would consider excellent contributors on each topic. There are several possible criteria for naming a topic:

- A research problem area may now be stimulating many fruitful investigations, and you believe it can become even more productive.
- A research problem area may, in your opinion, be of great importance and ripe for substantial advances even though the research has thus far been disappointing or thin.

- Rather than a particular substantive problem area, there may be a broad methodological or theoretical gap that needs focused attention.
- There may be need for new infrastructural arrangements to facilitate research, such as regional or national laboratories, data centers, communications networks, specialized fellowships, etc.

The Committee plans to form working groups, largely outside its own membership, on each topic or closely related group of topics selected for further analysis. These analyses will highlight relevant research issues, prospects, needs, and opportunities foreseen in the coming decade; inventory the present major research assets; and recommend new facilities, resources, or programs as needed, explaining how and why these recommendations would capitalize on the research opportunities foreseen. These working group reports will become the basis for the Committee's final report.

A form is enclosed for you to write a few lines describing a research problem area, resource need, or other working group suggestion; to indicate which criteria it meets; and to name several outstanding investigators or other persons who would be valuable contributors on this topic. You are welcome to spell out your advice at greater length if you wish or to comment on the individuals named. You are also welcome to send more than one form. We enclose as guidelines some sample descriptions.

On behalf of the Committee, its sponsors, and the organizations with which it is affiliated, we thank you for your help in this venture. Additional details are covered in the enclosed project summary. If you have any questions, please do not hesitate to contact either one of us or the committee's professional staff, Dean Gerstein and Sonja Sperlich.

Sincerely,

R. Duncan Luce   Neil J. Smelser

Enclosures

NATIONAL RESEARCH COUNCIL
COMMISSION ON BEHAVIORAL AND SOCIAL SCIENCES AND EDUCATION
2101 Constitution Avenue Washington, D.C. 20418

COMMITTEE ON BASIC RESEARCH IN
THE BEHAVIORAL AND SOCIAL SCIENCES

(202) 334-3017

R. DUNCAN LUCE, Co-chair
Harvard University

NEIL J. SMELSER, Co-chair
University of California, Berkeley

MEINOLF DIERKES
Science Center Berlin

JOHN A. FEREJOHN
Stanford University

LAWRENCE M. FRIEDMAN
Stanford University

VICTORIA A. FROMKIN
University of California, Los Angeles

ROCHEL S. GELMAN
University of Pennsylvania

LEO A. GOODMAN
University of Chicago

JAMES G. GREENO
University of California, Berkeley

EUGENE A. HAMMEL
University of California, Berkeley

LEONID HURWICZ
University of Minnesota

EDWARD E. JONES
Princeton University

GARDNER LINDZEY
Center for Advanced Study in
the Behavioral Sciences

DANIEL L. McFADDEN
Massachusetts Institute of Technology

JAMES L. McGAUGH
University of California, Irvine

JAMES N. MORGAN
University of Michigan

RICHARD L. MORRILL
University of Washington

SHERRY B. ORTNER
University of Michigan

KENNETH PREWITT
Social Science Research Council

BARBARA G. ROSENKRANTZ
Harvard University

NANCY B. TUMA
Stanford University

ALLAN R. WAGNER
Yale University

DEAN R. GERSTEIN
Study Director

March 1985

TEN-YEAR OUTLOOK ON RESEARCH OPPORTUNITIES IN THE BEHAVIORAL AND SOCIAL SCIENCES

The Committee on Basic Research in the Behavioral and Social Sciences was established in 1980 to evaluate and improve the vitality of research in the behavioral and social sciences. The committee is housed in the Commission on Behavioral and Social Sciences and Education of the National Research Council.

The committee's first two projects concentrated on the present dimensions and past record of the behavioral and social sciences. An initial report, Behavioral and Social Science Research: A National Resource (1982), developed criteria and cases to assess the present value, significance, and social utility of basic research in these subjects. A second report, now being completed, derives from a November 1983 symposium commemorating the landmark report of the Presidents' Research Committee on Social Trends (1933). This retrospective symposium was entitled, "Knowledge in Social and Behavioral Science: Some Discoveries and Trends Over Fifty Years."

The committee is now engaged in a prospective study, a ten-year outlook on research opportunities in the behavioral and social sciences, in cooperation with the Social Science Research Council and the Center for Advanced Study in the Behavioral Sciences. The ten-year outlook is designed to study scientific frontiers, leading research questions, and new resources needed over the next decade, roughly 1986-1995, for rapid progress on fundamental problems in the behavioral and social sciences. The ten-year outlook study began in March 1984 and is supported by the National Science Foundation, Russell Sage Foundation, National Institute on Aging, National Institute of Child Health and Human Development, National Institute of Mental Health, and the U.S. Army Research Institute for the Behavioral and Social Sciences.

The National Research Council is the principal operating agency of the National Academy of Sciences and the National Academy of Engineering
to serve government and other organizations

In organizing the study, the committee sought advice from a broad spectrum of distinguished scientists in the senior, mid-career, and younger ranks. The committee asked approximately 2,400 individuals to recommend particular subjects that should receive intensive analysis, and invited the editors of 150 professional journals in the behavioral and social sciences to publish open requests for recommendations. The committee has received approximately 600 responses, providing more than 1,000 separate recommendations.

On the basis of these responses and its own judgment, the committee has formed 30 topical working groups to consider in detail current and prospective research opportunities. Each group is comprised of five to ten members chosen from outside the committee. For the most part, these groups are organized to reflect research problem areas rather than disciplinary or sub-disciplinary boundaries. Each area is characterized by one or more of the following criteria: recent, dramatic advances in research methods or theories; a convergence of interdisciplinary interests, indicating potential future concentrations of effort; enduring lines of work with continuing gains in knowledge. Although the committee believes each of these areas merits further analysis, these early selections do not bind the committee or preclude other considerations from entering into the ten-year outlook. An annotated list of the working groups is attached.

Each group will draft a working paper, which is to define priorities for new or expanded support of program initiatives, multi-user facilities, institutes, fellowships, or other "infrastructure" that may serve many lines of research. Each working paper is intended to give non-specialists a clear picture of the research objectives, practices, problems, and needs in the subject area. The working papers are not intended for separate publication, but for committee use in preparing its ten-year outlook report.

Following the receipt of working papers in mid-1985, the committee will begin drafting its report, which will assess leading research questions and resources needed to pursue them, and make priority recommendations for new scientific investments over the next ten years that represent, in the judgment of the committee, substantial promise of scientific and ultimately practical return. The report will, to the extent possible, reflect a consensus of leaders and members of the behavioral and social science research communities.

WORKING GROUP TOPICS AND CHAIRS FOR THE TEN-YEAR OUTLOOK ON RESEARCH OPPORTUNITIES IN THE BEHAVIORAL AND SOCIAL SCIENCES

SENSORY AND PERCEPTUAL PROCESSES

Norma V. Graham, Department of Psychology, Columbia University

This area includes research advances in the visual processing of color, depth, and motion; audition, speech perception and speech production; pain, touch, and other neuropsychological and neuromotor processes.

PSYCHOBIOLOGY OF LEARNING AND MEMORY

Richard F. Thompson, Department of Psychology, Stanford University

Investigators in this area study the brain mechanisms and systems that underlie new memory formation, lasting memory, learned behavior, and disorders of learning and memory due to brain injury and disease.

INFORMATION AND COGNITIVE SCIENCES

Saul Sternberg, Human Information-Processing Research Department, AT&T Bell Laboratories

Research frontiers in information and cognitive sciences cover parallel processing architectures, tutorial systems, temporal flows and representation of information, attention and multiple task performance, higher order processing skills, and control of motor activity.

LANGUAGE AND LANGUAGE PROCESSING

Frederick J. Newmeyer, Department of Linguistics, University of Washington

Developments and applications of linguistic theory address topics such as speech synthesis and decoding, aphasia, acquired and developmental dyslexia, cross-language acquisition and breakdown, sign languages, and grammatical universals.

DEVELOPMENT OF COGNITIVE AND SOCIAL COMPETENCE

Herbert L. Pick, Department of Psychology, University of Minnesota

Major lines of study include the formation of numerical skills, learning strategies, moral intuitions, and causal and ontological concepts in infants and preschoolers; procedural knowledge, constraints on induction, and metacognitive skills in older children; parental teaching styles; and cross-cultural differences.

HEALTH AND BEHAVIOR
David S. Krantz, Department of Medical Psychology, Uniformed Services University of the Health Sciences

> This research front covers behavioral variables affecting mental and physical health and illness, including studies of health promotion and disease prevention, psychoneuroimmunology, diagnostic and etiological correlates of illness, risk factors, coping and support, and health of the elderly.

AFFECT AND MOTIVATION
Hans C. Fibiger, Department of Psychiatry, University of British Columbia

> Investigations on affect and motivation are concerned with the psychobiological and psychosocial regulation of emotion, affective disorders, appetitive and addictive behaviors, emotional expression and development, stress, and aversive motivation.

SOCIAL INTERACTION
John F. Kihlstrom, Department of Psychology, University of Wisconsin, Madison

> Lines of advance include social cognition, group effects on individuals, reciprocal influence, construction and maintainance of identities, and transitions to enduring relationships.

GENDER STUDIES
Nancy M. Henley, Department of Psychology, University of California, Los Angeles

> Investigations using gender concepts and variables as central axes include studies on the historical transformation of gender systems, gender-linked socioeconomic inequality, language behavior, primate behavior, and other subjects.

INFORMATION AND DECISIONMAKING
Mark J. Machina, Department of Economics, University of California, San Diego

> New empirical and theoretical work is being done on individual decisionmaking under risk and uncertainty, group decisionmaking with incomplete or asymmetrically distributed information, framing of judgments, and prescriptive decisionmaking.

MARKET EFFICIENCY
Oliver E. Williamson, Department of Economics and School of Law, Yale University

This area includes theoretical developments bearing on the efficiency of markets in basic infrastructural commodities, such as housing, energy, transportation, and environmental resources, and empirical analyses using panel data and simulation models to study the effects of market-specific policies.

JOBS AND INEQUALITY
Frank P. Stafford, Department of Economics, University of Michigan

Core research topics include determinants of labor supply and demand, explicit and implicit contracts, unequal returns, relative wages, wage and employment discrimination, job composition, distribution of unemployment, and job creation.

MARKETS AND ORGANIZATIONS
Stanley Reiter, Center for Mathematical Studies in Economic and Managerial Science, Northwestern University

There is strong interest in the interaction between resource allocation systems such as administered, monopolistic, regulated and relatively unconstrained markets, and organizational incentives, structures, environments, populations, forms of information processing, internal coalitions, and marketing strategies.

COLLECTIVE CHOICE INSTITUTIONS
William H. Riker, Department of Political Science, University of Rochester

Studies of collective choice cover the properties of voting and agenda-setting in allocative institutions, game theory, institutional design, social choice theory, and properties of bureaucratic and centralized decision mechanisms.

EMERGENCE OF SOCIAL AND POLITICAL INSTITUTIONS
Douglass C. North, Department of Economics, Washington University

Research frontiers in this area include emerging forms of property, exchange, and transaction; long-term political development; and evolution of social institutions such as the modern market and the national state.

URBAN TRANSFORMATION AND MIGRATION
John M. Quigley, Graduate School of Public Policy and Department of Economics, University of California, Berkeley

Studies on the differential development of cities and regions involve analyses of local investment, innovation, productivity, resource endowments, environmental quality, and interregional flows of trade, capital, firms, and persons.

CAUSES AND CONSEQUENCES OF DEMOGRAPHIC CHANGE
Samuel H. Preston, Population Studies Center and Department of Sociology, University of Pennsylvania

Some important topics of interest are intergenerational relations in aging industrial states, formal and mathematical demography, changing household patterns, fertility and population determinants, and effects of population policy.

FAMILY AND DOMESTIC RELATIONS
John Modell, Department of History and Philosophy, Carnegie-Mellon University

Investigations in this area are concerned with the processes of family formation, decisionmaking, and stability/instability; shifts in family structure; interpersonal processes, and relations to social support systems.

FORMAL AND LEGAL PROCESSES
Robert Kagan, Department of Political Science, University of California, Berkeley

This subject includes local and international comparative studies on the proliferation of legal institutions, the due process revolution, the bureaucratization of public and private life, and forms of dispute resolution.

CRIME AND VIOLENCE
Alfred Blumstein, School of Urban and Public Affairs, Carnegie-Mellon University

Studies of crime and violence involve theories and empirical evidence on penal deterrence, sanction systems, criminal justice operations; and the sociological, demographic, and economic correlates and dimensions of crime, deviant behavior, and family violence.

RELIGION AND POLITICAL CHANGE
Daniel Levine, Department of Political Science, University of Michigan

Current research on modernization and secularization emphasizes the influence of religious activism on political processes, including such cases as Islam in the Middle East and Catholicism in Latin America.

CULTURE AND IDEOLOGY
James W. Fernandez, Department of Anthropology, Princeton University

This area is concerned with publicly transmitted systems of belief and sentiment, particularly the nature of symbolic consensus, the distribution and "naturalness" of non-empirical beliefs, cultural transformation and dominance, and the growth and transmission of ideas within social contexts.

INTERNATIONALIZATION OF SOCIAL, ECONOMIC, AND POLITICAL LIFE
Peter B. Evans, Department of Sociology, Brown University

Internationalization processes involve the diffusion, production, and trade of technological, cultural, and medical knowledge and commodities; international capital and labor flows; and local and trans-national political concomitants.

INTERNATIONAL CRISIS MANAGEMENT AND SECURITY STUDIES
Robert Jervis, Institute of War and Peace Studies, Columbia University

Research in this area covers the behavioral and conceptual dimensions of international arms competition, risks of war, theories and tests of deterrence, repeated-play games, and asymmetric information.

MACROECONOMIC POLICY
Stanley Fischer, Department of Economics, Massachusetts Institute of Technology

Directions of current work include behavioral microfoundations such as consumer savings and expectations; public finance; the impact of housing, pension, and other policies on macroeconomic performance; and use of advanced time series methods, models of expectations formation, and post-"oil shock" data to test contrasting basic theoretical assumptions.

SOCIAL STUDIES OF MODERN SCIENCE AND TECHNOLOGY

Daniel J. Kevles, Department of the History of Science, California Institute of Technology

Research interests in this area include the development of modern research communities, construction of scientific institutions, the production of knowledge and technology, and forms of interaction between scientific and industrial enterprise.

SOCIAL KNOWLEDGE PRODUCING INSTITUTIONS

Theda R. Skocpol, Department of Sociology and Department of Political Science, University of Chicago

This area of study addresses the relevance, application, and control of social and behavioral knowledge; the modern history of the social and behavioral sciences; and the relations of social and behavioral research institutions to the state.

LARGE-SCALE DATA BASES

Warren E. Miller, Department of Political Science, Arizona State University

Problems in data-base construction and utilization include the availability of data aggregates at various levels of analysis, development and use of longitudinal data bases, relations between data files, and centralized information about research data.

STATISTICAL ANALYSIS

John W. Pratt, Graduate School of Business, Harvard University

Research opportunities in statistics involve topics such as graphical techniques, spatial data analysis, sampling, forecasting evaluation methods, meta-analysis for combining results of different studies, use of advanced computational capacities, and statistical theories of risk assessment, quality control, and productivity.

MEASUREMENT AND SCALING

A. Kimball Romney, School of Social Sciences, University of California, Irvine

Investigations in this area include scale types and axiomatic characterizations of measurement structures; geometric and multidimensional scaling; and problems of retrospective validity, category standardization, and context effects in survey measurement.

Mr. **WALGREN**. Thank you, Dr. Luce.

[A biographical sketch of Dr. Smelser follows:]

DR. NEAL J. SMELSER

Neil J. Smelser is University Professor of Sociology at the University of California, Berkeley, where he has been a faculty member since 1958. He pursued undergraduate work in several of the behavioral and social sciences at Harvard University and Oxford University, and completed his Ph.D at Harvard in the Department of Social Relations in 1958. Among his published works are Economy and Society (with Talcott Parsons), 1956; Social Change in the Industrial Revolution, 1959; Theory of collective Behavior, 1962; Comparative Methods in the Social Sciences, 1976; Sociology, 1981, 1984. He has served as editor of the American Sociological Review, and as Vice President of the American Sociological Association.

Professor Smelser has held a variety of administrative positions in the Sociology Department, the Institute of International Studies, and the central administration at the University of California, Berkeley. Between 1982 and 1984 he served as Chair of the Berkeley Division of the Academic Senate, and he is scheduled to serve as Chair of the Universitywide (nine-campus) Academic Senate in 1986–87.

Since 1980 Professor Smelser has served first as member (1980–82), Chair (1982–84), and now as Co-chair of the Committee on Basic Research in the Behavioral and Social Sciences (National Research Council). He was co-editor of its first publication, Behavioral and Social Science Research: A National Resource, and its forthcoming (November 1985) publication, Fifty Years of Discovery. He is a Fellow of the American Academy of Arts and Sciences and a member of the American Philosophical Society.

STATEMENT OF DR. NEAL J. SMELSER, UNIVERSITY PROFESSOR OF SOCIOLOGY, DEPARTMENT OF SOCIOLOGY, UNIVERSITY OF CALIFORNIA AT BERKELEY, BERKELEY, CA

Dr. **SMELSER**. Mr. Chairman, members of the committee, my name is Neal Smelser. I am a professor of sociology at the University of California at Berkeley. My areas of research have included studies of institutional change. I am a student of social movements. I have also done work in sociology of higher education. I have been affiliated with the committee on basic research since its beginning in 1980 and at the present time I am cochair of that committee with Professor Luce.

He has described the stage of the work we are in, so I won't repeat that, even though I say a few things about it in my written testimony. My comments will be mainly supplementary to those that have been made so far. Both these gentleman are psychologists, among other things, and I will try to identify from the work that our committee has done several examples from the more social organizational or institutional side of life, in order to give you an occasion of some of the exciting areas of research that have been going on there.

I will be very selective, and I will add the disclaimer that Professor Luce made, that my selection of these examples was a choice on my part, and did not necessarily commit our committee to this final choice and emphasis. We have a great range of studies—areas of studies that we have identified as being exciting, novel and promising, which I won't be able to touch on—studies of markets and organizations, studies of jobs and inequality, urban transportation, religion and political change. The three examples I am going to develop briefly are, first, the study of criminal careers; second, the study of the process of collective choice; and, third, what I call the increasing internationalization of social, political and economic life.

With respect to the study of criminal careers, we are obviously now talking about a range of social problems that are perennial and in some degree interactable and which demand not only understanding, but also the development of effective methods of prevention in the larger society.

Sociologists and criminologists in recent years have established that while juvenile delinquency is a very widespread phenomenon in society, the bulk of the problem of serious crimes—I am talking about rape, murder, burglary—stems from a relatively small number of career criminals. Furthermore, it has become apparent that by and large the typical pattern is for individuals to be involved in criminal activity in the teens and early 20's, but then the drop-out rate from the world of crime begins to take effect, and only relatively few people contribute to the large number of serious crimes in society.

Now, this calls then for a view of crime not so much as an item of behavior, but the study of the actual career of the criminal of a period of time. That is to say what are the decisive moments at which a life of crime begins? What are the factors that sustain this kind of behavior, and thirdly, and as important, what are the kind of factors that contribute to the termination of a criminal career?

I will give you a little bit of an idea of some of the research that has been done. The first advances have been methodological in character. We have gotten a little more sophisticated about the measurements of crime than we once were. Normally speaking, in the past, we have relied on police reports for our knowledge about crime rates, but these, even when codified, have proved to be notoriously inaccurate and usually understated.

The two methods that have been used to supplement this has been the so-called victim interview; that is to say, instead of looking at police reports, you interview a sample of people and ask them if they have been a victim of crime in their life, and what kind of crime and how often and so on. And we get vast differences in the rates that are reported in the police records when we take these interviews of victims. Usually, the rates are much much higher in the victim interviews. Or in interviews that are made of people asking if they have ever committed a crime, and once again we get variations in rate of criminal activity according to this measure as well. All three of these measures are inexact and all of them have their frailties. By comparing and compiling them, we get a rather better idea of the patterning of crime in society.

Now as I indicated, criminal activities certainly are very closely associated with life cycle, with the highest crime being committed in the late teens and 20's as far as collective crime rate is concerned. What about some of the factors that influence entry into crime?

Well, family background factors appear to be quite important. We have some correlational studies that suggest that in communities with very high divorce rates, we also have higher crime rates, though we haven't been able to establish whether divorce itself is a distinctively causal factor.

We have had studies which suggest that the factor of separation in a family is not necessarily conducive to the entry into criminal activity, but that parental conflict which may lead to divorce may be the decisive factor; that is to say, we have a higher rate of crime among adolescent and young people whose parents have been divorced than we do in those cases where the separation has been through death, which suggests that conflict is the more decisive factor here than separation as such.

With respect to criminal activity, those factors that sustain criminal activity, the strongest evidence points to narcotic drugs; that is to say, the association between drug use and criminal activity with users having much higher rates of crime than nonusers. What we really need to look at more systematically is the influence of key life events—marriage, getting a job, going into the military, that sort of thing, and to assess the impact of these on criminal activity.

One of the things that emerges here—this is to supplement Professor Luce's comments about longitudinal studies—is that the really most effective way to study the crime careers is to take a large systematic sample of children and young people and track them over a period of years, if not decades, so that we can pinpoint not only the pattern of criminal activity but also those critical life events that are most decisive in determining the entry into, the duration of and the termination of criminal careers. But the paradox is that we simply do not have the capacity in our funding mechanism to do this kind of research.

As Professor Luce pointed out, most funding circumstances also are brief, a few years at most, and this type of research which we know, and criminologists concur in, is in the best type of research to be done on studying the crime career, and simply cannot be executed at the present time under present funding structural arrangements of funding.

The second illustration is in the area of collective choice. Professor Simon made some comments about what we are going to learn about the process of individual decisionmaking, and there is a parallel line of research in the area of how decisions are made in a collective setting. The kind of starting point for this comes out of collective interaction. It is based on the assumption going back to classical economics, which is that when individuals make decisions in the collective setting such as the market, they follow out their own individual utilities, and behave rationally, and the functioning of the market is a kind of sum total of those individual decisions that are made.

Now this model has, in a variety of complicated ways, been modified in collective and social theory in past decades, and a great deal of theoretical and empirical work on exactly how people's choices and their decisions are modified in different collective settings has been undertaken.

One line of theoretical work was initiated by Paul Samuelson in his so-called free rider theory. People will distort their own preference for public goods in order to reduce the burden they may have to have by way of taxation, if they stated their true preferences for public goods. This has led to a lot of theoretical work on attempting to discern those conditions under which people will change their preferences for publicly valued goods.

Another line of decisionmaking about collective choice, decisionmaking, has to do with the importance of organizational structure on the way in which people come to a collective decision. Actually Professor Luce has already provided you with an example of that in his discussion of jury decisionmaking; that is to say, the rules whether it is a rule for unanimity or a rule for majority vote, will in fact change the way that people actually make up their minds and the decisions they come to collectively. And this is a small example of a vast range of research that is now going on in which both experimentally and in field studies scholars are looking at the effect that organizational context has on individual expectations and on the kinds of decisions that are made in collective setting.

We are modifying our model of collective decisionmaking by taking into account institutional stucture and its influence on decisionmaking. There have been studies of legislators, with comparative analysis of what difference it makes that the legislators are broken into committees; what impact agenda-setting, what impact different rules for preparation and decisionmaking have on the behavior of legislators.

In addition, there have been a good deal of comparative studies on the types of decisions that emerge from different voting systems. That is to say, examining the impact of plurality versus majority elections in terms of the kinds of decisions made in relation to minority groups in the society. And all this is to say that the procedures, themselves, will have an impact on the decisions that individuals and collectivities will make.

One line of research having to do with institutional context is experimental. That is to say, in an experimental setting, the experimenter will change the rules for decisionmaking in the experiment. That is to say, he will define a market situation, for example, as one of bartering, one of setting of contracts, and will simply—or one of a free market, and he will examine then the ways in which individuals' expectations and utilities and behavior change according to how one changes the institutional rules of the game. And a certain amount of exciting empirical work has been going on along these lines in a number of universities.

What I would like to point out about this particular line of research is that, first of all, a good deal of the experimental work is simply not being done because of lack of funds for training of subjects or replication of studies, and so on.

The second observation I would like to make is that many of these studies in this area are interdisciplinary, that is to say they are being conducted by psychologists, economists, political scientists, and sociologists. And the boundaries between the fields become quite fuzzy, because they're focusing on a particular problem of collective choice.

But much of the funding is done by disciplinary panels, judgments by disciplinary panels, and a lot of this work is likely to fall through the cracks, if you will, because of the disciplinary organization of giving and funding on the part of both Government agencies and private foundations.

The third example is one that is familiar to all of us, and that has to do with research on the impact of the increasing internationalization of social, economic, and political life. Yesterday's paper contained a lead article on the ambiguities now associated with continuing these massive loans to Mexico because of the performance of its economy.

This morning's paper, we read that for the first time the United States has become a debtor nation. These are simply daily recurrences of the fundamental fact that the interdependency of the world, both economically, politically, and socially, has been increasing, and this is evidence to management, to labor, to consumers, to bankers and governments the world over.

This internationalization is not only trade, it's the internationalization of capital, the internationalization of production, through the development of multinationals, and the internationalization of labor, that is to say, the use—the striving of international productive firms for the cheapest labor carries themselves across national boundaries, and we're having an increased nationalization, if you will, of the labor force as well.

One of the implications of this increased interdependency, of course, is that—well, how shall I put it—that the decisionmaking that we normally associate with sovereign nations seems now to be in many cases to be slipping from the hands of those nations because of the importance of international forces. The most vivid example, of course, is in Third World countries whose fortune—whose weak economies are literally dashed when rapid price changes take place or when movements of capital or when monetary fluctuations occur. These economies are simply influenced by forces that are out of control of the governments that run those countries. And to some degree, this applies to the stronger economies as well, that is to say trade deficits, monetary fluctuations, price fluctuations such as the vast fluctuations in oil prices that we have experienced, all are forces outside the hands of sovereign governments, and in some sense, we have, you might call it, a contradiction between the functioning of the world economy and the capacity of individual states to deal with the instabilities that occur.

There are also implications from this increasing internationalization for the academic world, itself. If you look at the social sciences, political sciences, sociology, and anthropology, you will find by and large they have taken the state or the culture or the nation as the unit of analysis, and that's been sort of the foundation of our understanding of what collective social life is all about. But insofar as we have this increasing interpenetration of the world, of nations, the nation as a unit becomes somewhat less salient, and we really have to revise our own perceptions of the unit of analysis that is most important in determining the fates of countries and peoples.

Well, scholars from economics, sociology, and political science have begun to develop some insights into the sorts of processes that are triggered by the increasing interpenetration of production and capital and labor in the world, and that focus has been given to the impact that multinationals have on the host states in which they penetrate, the different kinds of symbiotic relations that grow up, not only the opportunities, but also the burdens that are imposed by the massive, massive patterns of loans that have developed in the past decades, and on the impact of the internationalization of labor on the capacity of laborers to unite into labor unions inside into the patterns of migration that have been developed by internationalization of the economy.

Well, these are just a few samples. I would like to make a similar observation about this area of study as well, namely, that the structure of funding is not particularly conducive to further research on these phenomena of internationalization. This is an area in which collaborative research is particularly important not only among people from different disciplines but also international collaboration, collaboration between scholars from different countries, because we are dealing with international phenomena. But if you look at the structure of funding, this is virtually impossible now to execute because of different restrictions, again because of the lack of opportunity for really effective longitudinal studies and because of the funding policies that tend to limit funding to nationals of this country.

Well, what I have really tried to do, without hopefully being too repetitive with my colleagues, is to indicate there are a few areas of really vital and exciting research I have been able to detail for you and that they have tremendous promise, but that in each case, the way we have now to go about funding these areas of research definitely inhibits and restricts the degree to which they can be carried forward.

Thank you very much.

[The prepared statement of Dr. Smelser follows:]

September 17, 1985

Mr. Chairman and Members of the Committee on Science and Technology:

My name is Neil J. Smelser; and I am Professor of Sociology at the University of California, Berkeley. I am also co-chair of the Committee on Basic Research in the Behavioral and Social Sciences of the National Research Council. The general charge of the Committee on Basic Research is to provide advice and counsel from the academic community to various agencies involved in supporting and sustaining other kinds of interactions with the behavioral and social sciences community. This was in direct response to concerns voiced in Congress, including this hearing room, concerning the value, significance, and social utility of basic research supported by federal agencies. I have been affiliated with the Committee on Basic Research since its creation in 1980, serving first as member, then as chair, and now as co-chair.

About two years ago the Committee began to develop a ten-year outlook for the behavioral and social sciences, and, in this connection, to identify a number of the areas of greatest excitement, ferment, accomplishment, and promise in these sciences. Such areas, as identified, would provide a basis for recommending new priorities to agencies responsible for funding these realms of scientific inquiry. Our committee report is scheduled to be completed in 1986.

A very important early decision on our part was to tap the intelligence of scholars-at-large. In that connection we surveyed approximately 2500 behavioral and social scientists--running the gamut from scholars engaged in studying the biological bases of brain functioning at one end to those studying the workings of the international economy on the other. We asked these scholars to identify the leading edges of work in their areas; in all we received well over 1000 suggestions--some repeated, of course--from our respondents.

After receiving these responses, the Committee engaged in a complex process of grouping, synthesizing, accepting, and in some instances, discarding them, and, on the basis of that process, established approximately 30 special working groups of experts in areas we had identified. We commissioned each group to provide a report of approximately 20 pages to be submitted to the parent Committee as raw material for its final report. We are delighted to report that we have had a 100 percent return rate from our working groups, and that the Committee is now ready to move into its final phase of work.

As you might anticipate, the results of this extended survey are extraordinarily rich. We have discovered the most exciting array of research in area after area, discipline after discipline, and, above all, work of an unprecedented interdisciplinary character. I trust that Professors Simon and Luce and one-time Professor Moynihan have given you illustrations of such work in the behavioral sciences, sociology, and economics. I would like to reach into another range of topics, to give you an illustrative sample of exciting and novel research at the social-organizational and institutional levels.

At this more macroscopic level, as well as in the other levels covered, the range and depth of the research cannot possibly be covered in the scope of a brief testimony. We have impressive working-group reports in the areas of gender studies, markets and organizations, jobs and inequality, urban transformation, religion and political change, studies in modern science and technology, and others, which I simply will not be able to cover. By way of illustration, I offer three examples I personally consider very salient--salient with respect to ground covered, promise for the future, and relevance for planning and policy. These examples are (1) the study of criminal careers; (2) the study of processes of collective choice; and (3) the study of the increasing internationalization of social, political, and economic life.

The study of criminal careers. At all levels of society--from the concerned individual citizen to the federal government--dangerous and violent crime has long been consistently identified as among the nation's most serious problems. The problem has proved to be not only serious but also in some degree intractable, demanding deeper understanding in order to develop more effective methods of prevention and control at all levels of social organization and law enforcement.

Modern research by sociologists and criminologists has now fairly well established that while juvenile delinquency is a very widespread phenomenon with respect to proportions of youth who engage in violations, the bulk of the problem of "serious crimes"--murder, rape, aggravated assault, etc.--is due to a relatively small number of "career criminals." This recognition has called in turn for systematic research into those developmental processes which produce these kinds of criminals--and which produce people who do *not* engage in crimes--in short, into the criminal *career* as a lifelong process. Some insights into the criminal career have already been generated, but, at the same time, scholars realize that their present research capabilities are limited and can be improved.

What has been discovered? First, on the methodological side, improvements in the measurement of both individual and aggregated crime rates have been achieved. This has been done mainly in the development and refinement of two types of measures of crime in addition to those of the police-generated FBI Uniform Crime Reports. These two new types are self-report surveys of individuals regarding their involvement in criminal activity, and surveys of victims of crimes. Each of these, like the Uniform Crime Reports, has its limitations, but using all three sources to shore up these respective weaknesses has increased our sophistication, knowledge, and confidence concerning the incidence and pattern of individual crime rates.

On the basis of careful studies of these improved measures of criminal activity, a number of preliminary findings about the criminal have begun to emerge. First, it has been learned that a very high percentage of American males--almost 50 percent--can expect to be arrested for some kind of non-traffic offense at some time in their lives. However, it has emerged that the great number of offenders commit only one or a few crimes, and a small number commit crimes at a very high rate. Looking at the population as a whole, the most active period for offending per general age group is the late teens. But there is a heavy drop-out rate from adolescent criminality. By the late 20s and early 30s, those committing offenses are largely the serious career criminals who are persistent high-rate offenders.

What are some of the factors most likely to affect a criminal career in its various phases? Research here is promising, but by no means decisive. With respect to entry into a criminal career, family influences seem to be the ones that appear most consistently in the ongoing research. In correlational studies, communities with high divorce rates have high rates of entry into criminal careers. But we don't know if it is actually the children of divorced parents in those communities who are responsible. One finding does suggest that the existence of marital conflict surrounding divorce rather than the fact of losing a parent matters most in this connection: children from single-parent families due to a parent's death are less likely to be involved in offenses than children of divorced parents.

With respect to rates of criminal activity during the active portion of a criminal career, a powerful influence appears to be narcotic drug use, with regular users having a much higher rate than those who are not users or who have discontinued use. With respect to the termination of a criminal career, much less is known, though drug usage appears to be a factor here as well. What needs to be examined much more systematically is the effect of key life events such as marriage, employment, joining the military, moving to a new community, and the like.

One paradox emerging from this developing research on criminal careers is that while some important and suggestive findings are emerging, those who are conducting this research are handicapped in doing the most effective possible kind of research. What I think is required--and there are many in the field of criminological research who concur--is to observe an appropriate sample of children and adolescents carefully over a period of years, perhaps even decades, to determine not only which of these will and will not engage in criminal activities, but also to pinpoint those life events that appear to be most important in initiating, or in terminating a criminal career. Such longitudinal information would also be invaluable in determining the effect of societal interventions in the criminal process--interventions such as probation, imprisonment, educational programs, community development programs, and the like.

One further aspect of this paradox is that while longitudinal studies of this sort seem to be clearly the most effective kinds of research that should be undertaken to expand our knowledge of the criminal career, funding for these kinds of studies is not now available either from the federal government or private foundation sources. Part of the reason for this is that such research is expensive to execute, but not much more so than other forms of research. Another, perhaps more important reason is that social science research funding agencies operate on short cycles of funding--a few years at most--that virtually prohibit a firm assurance of support for studies of the duration and effectiveness required. In the case of this illustration, then, we have a combination of (1) the appearance of very revealing and valuable research; (2) a challenge and opportunity to enhance this research appreciably; and (3) a serious need for resources and funding structures to permit its execution.

The starting-point for talking about collective choice is the model of classical economics, going back to Adam Smith. This holds that behavior in a collective setting--for example, a market--is a matter of a number of individuals with fixed preferences, each one acting rationally to maximize his or her utility. If everyone behaved consistently in this manner, an optimum and stable equilibrium, such as a clearing of the market, would be attained (the invisible hand). Modern choice theory has modified this simple but powerful view of the collective process by investigating how preferences themselves can be modified or misstated in the collective process, how individuals not only take their own but also others' preferences and strategies into account (game theory), and by modeling the network of rules and procedures (and this means, in fact, institutional structure) in which collective interaction takes place.

One line of inquiry was initiated in the theoretical work of Paul Samuelson, who, in his formulation of the notion of the "free rider" problem, posited that individuals might understate their actual preferences for public goods (such as public parks or even schools) in order to reduce their personal tax burden. One line of theoretical development that has arisen from this formulation of the problem is that economists and others have begun to think about how the same actors in different institutional arrangements will place different values on public goods and therefore produce different public decisions. This kind of thinking is directly relevant to the structure of taxation and tax incentives.

Another line of influential new thinking about collective choice involves decision-making in the context of an organizational structure with a division of labor and rules for decision-making. For example, it is evident that a legislature is not a periodically-renewed assemblage of individuals who deliberate and decide everything in a completely free and ad hoc manner. Rather, legislatures are characterized by systems of committees, institutional leaders, people responsible for setting and adhering to agendas, and a whole complex of procedures that determine in what sequence and under what conditions decisions can be made and can be regarded as binding.

What specific lines of research have been stimulated by these perspectives? One line of research has been into the empirical conditions that make for cooperative rather than uncooperative behavior in group settings. It has been posited--and confirmed in some degree--in empirical settings that *if interaction of individuals is repeated over time* (as contrasted with one-shot interactions), and if one or more members of a group has a reputation for cooperation--and for retaliating if cooperative behavior is not forthcoming--even if there is some uncertainty about retribution for non-cooperating, then everyone is more likely to engage in cooperative behavior. These kinds of findings have obvious implications for the design of organizational structures, such as classrooms, prisons, and working settings.

Researchers have also made both natural and experimental inquiry into the place of agendas and agenda-forming as an integral part of the decision-making process rather than as a simple means to guarantee order. While it would be too much to say that in general the one who controls the agenda controls the outcome, we know now with certainty, and in settings where this would not be at all obvious, that the agenda itself is an important part of the decision-rule process and an important element in shaping both domination and opposition in organizational settings.

Considerable research has been conducted on the effect of different kinds of electoral systems and how fair each might be for different social groups. In the United States, Britain, Austrailia, and Canada--unlike Continental Europe and most other areas of the world--the most common election procedure is plurality. While single-member district plurality elections predominate at the national and state levels in the United States, multimember district elections using plurality are the most common election type at the municipal or county level. Plurality multimember districts have come under increasing attack in federal courts on grounds that they dilute the voting strength of racial and linguistic groups. Testimony based on social choice theory has played a critical role in these challenges to multimember districts. Similarly, when some local legislatures in New York attempted to use weighted voting systems, testimony involving tools derived from social choice theory has been used to judge the fairness of such systems in terms of "one person one vote." Provisions for majority run-off elections (for example, in some Southern states) have also come under challenge as racially discriminatory in their effects. There now exists a body of research completed during the past decade on the properties of run-off election systems. While the results of this kind of research cannot be translated automatically into recommendations for changes in constitutional systems or judicial decisions, certainly they provide highly pertinent empirical results that can be taken into account in attempting to devise the fairest kind of electoral systems.

One emerging trend in the area of institutional effects on decision outcomes is experimental research. Groups of individual subjects are brought together in laboratory settings and asked or trained to interact (usually in market situations) according to different preferences, rules of exchange, and amounts of information. These experiments permit very precise understanding of how behavior proceeds in different situations of bartering, auction, free exchange, and various types of contracting arrangements. In constructing such experiments, the experimenters are, in effect, building different institutions and asking, under experimental situations which can be replicated and checked for reliability and persistence, if there are different kinds of calculation, or "rationalities" that arise in these different kinds of structures.

I conclude this illustration with two personal observations on the study of collective choice, based on my reading of the materials. The first is that support for experimental studies--to properly equip laboratories, to recruit and train subjects, and to replicate studies--has been inadequate to fulfill the promise of these kinds of studies. The second point to be made is that the growth of interest and research in collective choice has been truly interdisciplinary in character, involving the efforts of economists, political scientists, sociologists, and psychologists. But a great deal of funding in both the federal and private sectors is disciplinary in orientation with respect both to the focus of the program and the composition of reviewing panels. These structural arrangements mean that many of the interdisciplinary developments, such as that just reviewed, may fall between the cracks. I would hope that more flexible funding arrangements might be envisioned in the future so that these kinds of advances can be more readily furthered.

The internationalization of social, political, and economic life. In the late twentieth century, the world witnessed a phenomenal increase in its international connections and interdependencies. Ask only management and labor, whose common apprehensions about competition from foreign imports have united them in a kind of common concern if not common interest. Ask bankers, whose exposures to fluctuations in internal currency and confrontations with international debts are part of their daily diet. Ask consumers, whose homes and offices are truly a melange of products from around the world.

The facets of this increased internationalization are many. There is an increasing internationalization of trade; an internationalization of production, largely through the penetration of multinational corporations into the various nations of the world; an internationalization of labor, as increasing numbers of firms direct their activities to areas of the world where it is most economically found; and internationalization of technology, as nations strive for advantages in the competitive struggle; and an internationalization of culture, as increased interaction among nations calls for the intervention of new understandings and the penetration of the mass media brings an increasingly shared exposure to cultural themes carried by television, radio, and the press.

The increasing internationalization of the world means that, while the political structure of the world is still mainly along lines of sovereign nation-states, these nation-states may be losing, more and more, control over their own destinies because of international penetration. This is most evidently the case in many third world countries, where the penetration of multinational corporations, the buildup of huge foreign debts, and the international fluctuations of prices and trade patterns play havoc with their weak economies, and tend to overwhelm national governments' efforts to control such forces. But it also applies to the more advanced industrial nations struggling with different patterns of foreign competition, dependency on imports not available domestically, and trade deficits.

The research implication of the increasing internationalization of the world is that validated social science knowledge that has been built, by and large, on the foundation of studying "the nation" or "the culture" or "the area" as a primary unit of analysis, may be becoming outmoded. Insofar as international forces become more salient in the determination of nations' fates, knowledge about nations (which could formerly behave as autonomous units with their own autonomous fortunes) may be increasingly archaic. New conceptualizations and theories of the nation-in-the-world appear to be called for, and some of the fundamental principles of earlier economics, sociology, and political science may have to be revised in light of the changing realities of the world situations.

In the past decade or two, scholars from these disciplines have certainly begun to take account of these fundamental changes, and a growing amount of research is beginning to throw light on this new world situation. In particular, scholars have taken an interest in several dimensions of internationalization. One of the most important is the impact of international finance on the domestic policies of states. On the one hand the presence of international financial institutions--and international debts--has severely limited the options of states in controlling their monetary and investment policies. At the same time, however, the presence of massive amounts of foreign capital has given debtor states greater choice and autonomy and the chance to make greater blunders, in the short run. As an instance of research in this area I cite the work of Professor Gary Gereffi of Duke University, who has traced the contrasting investment strategies of Taiwan and South Korea, which moved early and aggressively into investment in export industries, with those of Brazil and Argentina, which relied for decades on a strategy of investing in industries whose products were meant to compete with imports. All these countries have been debtor nations, but their strategies and growth patterns have differed markedly.

Even more research has been conducted on the effect of the internationalization of production. The penetration of special kinds of international firms--those in the extractive industries, for example--may serve to direct if not actually skew the directions of economic development of third world countries away from what might be optimal in the long run. On the other side of the ledger, however, transnational penetration does offer substantial employment opportunities for the domestic labor force, and substantial opportunities for the governments of host countries to establish cooperative arrangements with the multinational corporations. Still other research suggests that penetration of multinational corporations into less-developed economies may have the effect of slowing efforts to achieve a more equitable distribution of income. And finally, the internationalization of production has lent new salience to the importance of migrant labor, the capacity of labor to maintain unions and other labor organizations across national lines, and to new forms of labor-management relations that cross international lines.

The study of the internationalization of the world has attracted interest and made strides forward, but at the same time is inhibited by current program policies and structural arrangements. The best and most revealing research on the phenomenon of internationalization is interdisciplinary in character and international in scope. The disciplinary character of funding inhibits this kind of collaboration. Even more serious is the virtual collapse of funding for collaboration between scholars from different countries. Current funding patterns in both federal and private agencies make support of such work difficult if not impossible. So we see an evident need for research is simply not well served by the present capacity of the supporters of research.

Mr. Chairman and members of the Committee, I believe my testimony to you today has been sufficiently straightforward--if not repetitive--in its message so that I need not summarize elaborately. I have made no effort to exhaust the multitude of developing and exciting areas of organizational-institutional research in the social sciences; indeed, in the interests of being as concrete and brief as possible, I have been very selective. But in each of the three cases I have been able, I hope, to have identified a common theme: Social scientists have been able to make clear strides in a number of research areas that call out for understanding and for knowledge that is relevant to those who are responsible for planning, making policy for, and contributing in various ways to a number of areas of social, economic, and political life; and opportunities for increasing this knowledge are evident; but present arrangements for supporting these further advances do not necessarily provide an adequate base for moving ahead as rapidly as we must.

DISCUSSION

Mr. **Walgren**. Thank you, Dr. Smelser. The point is well made, and the difficulty of making the support structure consistent with the potential areas of exploration that you have outlined is certainly going to be a challenge to the scientific community and to the Congress in view of the degree of support that we have—that science in general relies on from the Federal Government.

I wanted to ask at the outset for comments on the difficulty that the social and behavioral sciences have had in Government funding. I thought that when I came to this problem in 1980, or thereabouts, by chance through working with this committee, that like all things, that was a new phenomenon. But apparently that certainly is not so, and the controversy over the funding of the social sciences seems to run way back into the early 1950's, and I find that other Members of Congress who have gone on to other committees at this point were major figures in that controversy. And so someone like myself who comes later on certainly is not new to the area—or it is not a new question.

Is it a question of the political system not appreciating the product that the social sciences have to deliver? Is there a syndrome of not appreciating the messenger, in the sense, when the data that we have are criticisms, social criticisms and the like? To what do you attribute the difficulty and the controversy the social sciences have had that have resulted in the reductions of most recent years, but really individual areas of controversy in prior years?

Dr. **Simon**. I think there are several factors. One is that the issues the social sciences deal with are perhaps, on average, even basic social science, closer to our every-day lives than the issues of most of these other sciences, although it would be hard to say that about research on AIDS or cancer. Those are pretty close to life, too—maybe not as controversial.

Research on acid rain is a better example. You can find examples much more easily in the social sciences where the information we're gathering does bear on very controversial questions and where social science has to really be concerned about the line between research and advocacy. I think sometimes advocacy is perceived where research is done, but that's another issue.

I think that line is a little harder to draw in the social sciences, and many people are more skeptical about whether it can be drawn effectively in the social sciences than in natural sciences. Clearly, we wouldn't be here if we didn't think it could be drawn.

Second, the fact I mentioned that the social sciences deal with our own behavior, and we know a lot about our behavior. We get up every morning and watch it for 16 hours more or less before we go to bed again, and we watch our friends, and we watch everybody else. It's a little harder to establish a group of people who are supposed to be more expert on that behavior than the people, themselves.

Physicists had to deal with Geiger counters, were self conscious and made physics a little tough. So the social scientist has to develop evidence that sometimes flies in the face of common sense, and that shouldn't be taken without skepticism, without proof, but it does create a more difficult problem.

The third is, maybe partly as a result of the fact the social sciences have not been as adequately supported in our society as other fields, sometimes we don't have the aspirations we ought to have, sometimes we don't move the decimal far enough over, we say, "We can get $20,000 to do a study, let's do that kind of research," when we ought to be thinking about important problems of the sort that were mentioned here today, some of these longitudinal problems where we ought to really have that decimal place two or three points further to the left.

In my view, all three of those things have played a role in the difficulties we had in getting what seemed to us the kind of support that the sciences need and that society deserves.

Mr. **Walgren**. May I ask for other responses?

Dr. **Luce**. I think in the period shortly after the Second World War, the tendency was on the part of the physical sciences to be very skeptical of what was going on in the social sciences, and only gradually have they come to have one—some awarness of the difficulty of the problems that we're trying to deal with and with the fact that in fact some serious work is going on, and there is accomplishment.

I think one of the signs of the change between the Second World War and now was at that earlier time, the National Academy of Sciences, for instance, did not really recognize social sciences as a separate entity. They had some archeologists and anthropologists and some—experimental psychologists were very strongly on the physiological end, but that was it.

And in the late 1960's and early 1970's, an appreciable change of attitude developed, and it was partly, I think, stimulated by the experience of the National Research Council trying to give advice to the Government on various kinds of complicated problems, and many of those problems are as much social science in nature as they are physical and biological science. There's that interface of application where the social sciences really have an input, and gradually the National Research Council in various ways recognized this, and the social sciences became increasingly accepted.

But in the earlier period, which is when a lot of funding decisions had been made, setting up the National Science Foundation, the behavioral social sciences were simply not accepted by the national science community. I think that has undergone some change, but it's got a ways still to go.

Mr. **Walgren**. Dr. Smelser.

Dr. **Smelser**. One supplementary remark, Dr. Simon indicated the social sciences have to do with what we think we all know about, that is our own behavior and so on. I should point out the subject matter and mode of research almost inevitably touches on political and social cleavages in society and the major issues of the day that—that are struggled with by those responsible for leadership.

I have in mind, for example, the very controversial studies on intelligence and the implications of whether or not we seek out biological bases for intelligence or the social class bases for differences of intelligence, ethnic group differences in intelligence. This feeds immediately into lines of political conflict and cleavage in society, and as almost an automatic result, this kind of research is going to be controversial in some groups' minds, and it's inescapable, and because of that the research itself becomes controversial, even though efforts are made to carry it out on the most scientifically respectable basis.

Mr. WALGREN. Dr. Luce, you indicate that there was this lack of acceptance of the social sciences by the physical scientists, or at least those influenced levels of the physical sciences that were in the ascendancy as the Federal role in science developed rapidly in the 1950's. How would you argue to them that—and I think the answer I guess has been implicit in your testimony this morning, but would you—the argument to them, that these scientists—that these areas of scientific inquiry are legitimate and deserve to compete for funding certainly on the same range of legitimacy as the other sciences would, is there a short argument you would make to that point?

Dr. LUCE. Well, I think the argument has to be in part by an example, that you give examples of lines of research where systematic data and systematic theory are being developed, and attempt to convince the listener this is of the same ilk he or she is familiar with from the natural sciences. I think there has somehow been an image that we are different than the other sciences, our procedures are different. The subject matter is different, to be sure, but the procedures and the logic of the sciences are the same.

I don't—I guess I don't sense any great difference other than we're dealing with a certain class of phenomena. Now it is true, the phenomena are very complicated and that in many cases we don't have as well-developed theoretical idea as one has in the natural sciences, but in other cases—I mean, there are natural sciences where problems are comparably complex and the difficulties are also comparable.

Meteorology was mentioned as a case in point. Now, they have some detailed equations that we don't typically have in a lot of behavioral social sciences, although you will find if you start probing in economics, there are relatively explicit mathematical formulations. To be sure, subject to some controversy, but there is a good deal of consensus about the nature of the description of the phenomena.

In psychology, you have examples where there is a great deal of cumulative information and theory and others where things are much more problematic and where you get changes from one generation of scientists to another that people would question whether the science is cumulative, and this is a frequent charge, that the social sciences cannot be cumulative, and I think that's false. By and large, there is an enormous amount of cumulation, but there's a change sometimes in point of view that sweeps through the field and doesn't destroy the data that it previously collected, but sometimes alters radically how it's understood.

But that's not too surprising. It occurs in the natural sciences as well. The sweep of the paleotectonic idea took place some 10 years ago, or 15 years ago. The data hadn't changed appreciably. People began to see them in a different way, put them together in a different way, and understood them in a different fashion. I think you see a lot of that going on in the social sciences, and that is sometimes, when looked at logically, perceived as kind of nothing is accumulating. It's a fad, and something new has come in. I think this is a misunderstanding.

Mr. **WALGREN**. Let me ask if you could comment on the level of support for the social sciences and the behavioral sciences. I am not sure I am close enough to the field to make a good distinction, but in the background papers that were developed for the committee, the estimates are made of something in the range of $2 billion a year being spent by the Federal Government on the social sciences, social and behavioral sciences, which would, as I understand it, encompass both basic, applied and even something called development.

Can you give us any feel for the way the Federal funds are being expended now and whether there are ranges of those funds that are really not applicable to the goal of enhancing the social and behavioral sciences, at least in the development of the science sense of the word?

Dr. **SIMON**. When I last looked at the sets of figures like that, not real recently, I was struck by the broadness of the definition of what was included in social science in those figures, some things which I think many of us here would have difficulty in recognizing.

A major component in that is an important component, and that is data gathering. The U.S. census is a very expensive and a very important operation as a source of not only the information that was originally designed for, so that we know how to allocate congressional seats, but also for all sorts of basic information about the nature of our population.

If you took the census figures plus the Bureau of Labor statistics, plus other statistics, you would account for one large chunk of it; I don't know how much of it. Other things I find hard as describing as social science—activities in DOD, that would look to be as they are described, much more of a training nature or tryout nature of devices. Probably not more than 10 percent of that—I will let my colleagues, if they want to, guess at what the number is—probably not more than 10 percent would look at all like support for basic research in the behavioral sciences, and the numbers might be less. Perhaps you fellows have taken a look at those numbers lately, I don't know.

Dr. **LUCE**. One of the things in that background report that's been prepared for the committee is if you look at the major funding sources for basic research, which up until quite recently have been primarily NIMH and NSF, the real dollar figures have gone down in support over the last decade, and there's just no question it has decreased, and the number of active researchers in the field have increased during that period. So it's clear that we are hurting relative to what it was in the 1960's, let's say, the late 1960's. We're not hurting as much as we were in 1980, when cutbacks were enormous, as you well know, and had a great deal to do with it. There's been some reinstatement.

But the fact remains, we are poor right now and that a number of the things people want to do are not really feasible within the present budgetary limits, the kinds of large-scale projects that I was mentioning, of which I gave you a small sample, there are many others of a comparable sort. Those are not really very feasible at the moment, and I think the society has got to decide whether those kinds of projects are worthwhile, and, if so, then we're going to have to pay for them.

Mr. **Walgren**. Dr. Smelser.

Dr. **Smelser**. This is an observation on my part that I believe could be documented, and that is to say I believe in behavioral and social sciences there has been a falling off for the training and replenishment of personnel at the postdoctoral level over the past 10 years, and it isn't just a matter of research, it is a matter of maintaining that infrastructure which is essential for an ongoing enterprise as complex and demanding as that.

Mr. **Walgren**. Well, thank you.

Let me turn to other members. Mr. Brown?

Mr. **Brown**. Gentlemen, I didn't indicate anything at the beginning of your testimony about how important I thought your appearance here this morning was, but I think I can say now, based upon your testimony, that you have made an extremely important contribution to our better understanding of the opportunities in the social and behavioral sciences, and the importance of providing adequate support for them. I hope that this will be fully reflected in the work of the task force.

Dr. Luce and Dr. Smelser, you are working on this 10-year outlook, this NRC Committee that you head. As you know, we have set up in the law a framework for developing a 5-year outlook for science, which is periodically updated, in an effort to help us get some of the perspective I think you are getting with this 10-year outlook. I raise the question of, will these processes be integrated in some fashion so that we may have a formal assessment within the framework that we have established as to what these prospects in the social and behavioral sciences are? Will it be integrated with the work the Academy does in preparing the 5-year outlook?

Dr. **Luce**. I am not aware of any explicit attempt at the moment to integrate them. The 5-year outlook, covering the whole of all the sciences, and the behavioral sciences, usually appear as a small sample in each of those reports. This report of ours is to be more comprehensive, clearly. I would hope, but I don't know, whether in fact the next 5-year report would perhaps draw on our report, maybe give a synopsis of it; that I think remains to be seen. Everyone will wait to see what we produce. If we produce a reasonably desirable report, they will probably incorporate it to some degree.

Mr. **Brown**. I will be awaiting the report with some anxiety, and I trust there will at least be a cross-reference between the two reports so that individuals will pursue them at whatever their level of interest.

Let me touch on a delicate subject, the problems in gaining support for social and behavioral research, research in the social behavioral sciences. There really has been a perception—more or less strongly among Members of Congress—that you don't have real research in the social and behavioral sciences. You have gone a long way toward dispelling that notion this morning. I commend you for it.

But to give you a simple example, some of the history of research on intelligence—for example, intelligence was defined as that which is measured by an intelligence test, and that's not good science. You get into all these sticky problems having to do with trying to compare intelligence on class, ethnic groups; you don't know what you are measuring to begin with. That kind of thing has to be overcome, and it extends into many of the social science areas, and I perceive the beginnings from what you have said of recognition of this problem and an effort to approach it in what I think would be considered a more rational, at least, if not more scientific, way.

Do you wish to comment or tell me how far off base I am?

Dr. Simon. I don't think you are off base. In the social sciences, as in other fields, concepts start out fuzzy and they are clarified, and one of the tasks of research is to do that clarification. In Newton's time, people weren't clear as to what the difference between momentum and energy was. They had to get that straightened out before they could do good physics.

We have problems. Every frontier line of science has problems of making its concepts less fuzzy. But I think there's a special problem here, and maybe it relates back to Congressman Walgren's question, too, every kid who goes through high school in our society has a chance at least to find out what physics is, what chemistry is, what biology is. They may not like it when they find out, but they have a chance to find it out, and a lot of them do find it out.

There is very little chance in our secondary education today for people, even those who are essentially going to be social scientists to find out what the social scientists are. Social science courses in the high schools typically are related to modern social science. Psychology courses taught in high schools are rarely, rarely focused on modern scientific psychology; they're more concerned with helping kids understand themselves a little better and feel better about it, I guess. Maybe part of the lack of understanding, not only of natural sciences but of others in our society, of the core of good social science is due to the fact we have not very many opportunities to expose people to them, or certainly not the opportunities that the other fields have.

I don't think the situation is ever going to change as long as impressions of the social sciences are formed at second or third-hand or are formed on the basis of only those parts of the social science that become involved in political controversy. It's as if our whole view of biology was formed on the basis of its contribution to the abortion issue. That's maybe a strong way to put it, but there is a little bit of that flavor in the problem that we face in giving the impression of what really goes on in social science research.

Mr. Brown. None of the sciences are perfect. I can cite some other examples. But you are a social scientist.

Dr. Luce. May I make a comment on your question? We always have the situation in the social sciences of being asked to provide help in situations where we sometimes don't have the scientific basis to provide it, and in particular in a society where a lot of classification of people takes place, any kind of a scaling device that someone will come up with is likely to be put to applied use very much faster than many of the scientists involved would wish that it were.

And what you see then developing are misuses or potential misuses of a device such as the intelligence test, which I think were a little more carefully thought through than you indicate, but they still have a drawback of being kind of a one-dimensional view of intelligence. And in modern work, in cognitive psychology, the whole issue of what constitutes intelligences of various types is being explored, and questions of creativity as well as knowledge of reasoning abilities are being factored out in empirical, experimental fashion, and probably we'll have more subtle ways of dealing with these concepts. I certainly hope so.

Anyone who is admitting, for instance, graduate students into the university wishes we knew how to pick out those who have creative skills. You can pick out ones who are pretty bright, and we do that, and then out of that group of students, we find relatively few who go on to be creative. We don't know how to pick those people out in advance, they pick them out after the fact. Your definition of creativity is when they produce a creative piece of work. We just don't know how to identify them.

But those kinds of problems are being worked on, and while they're not yet receiving a lot of publicity, there's really quite an active set of research in this area.

Dr. **Smelser**. I have a comment. This has to do with what I would like to describe as our increasing sophistication in our area of survey. Early surveys, including the census, are based on the premise the best way to find out something about a person's opinion is to ask them, and there is an assumption built in there they'll tell you the truth and that you have an accurate measure.

Well, this has proved to be—this range of assumptions has proved to be wrong, and we have been able to show, for example, that in a survey even on relatively neutral opinion items, it makes a difference whether the interviewer is a man or woman, it makes a difference of the age of the interviewer, it makes a difference as to whether the interviewer is wearing a business suit or dressed like a hippie. All of these have become systematic measures of error, but the point is to react to those by seeing what those distortions are. There is no true measure of the attitude.

The point is to become more and more conscious of the kinds of measurement error that are developed by the process of measuring itself and by the variations in the mode in which it is being measured, and I can say our level of sophistication in this area is qualitatively greater than it has been in the past.

Mr. **Brown**. I am well aware of that. I really was sort of pursuing the issue which Mr. Walgren raised earlier as to what accounts for some of the reasons for low levels of funding for research in this area as compared with the physical sciences, for example. Those reasons are being overcome, very obviously, as a result of the phenomenon which you have discussed.

You raised some other important questions, the progress in the science now requiring large scale, let's say, multi-disciplinary centers with computational capability, as is true in the engineering and physical sciences. What can we do to get this? I think you are going to have to emphasize the need for it in your testimony and in your 10-year outlook.

But you may have to couple this with something else, also, because this situation didn't arise in the physical sciences and engineering out of pure eleemosynary attitude on the part of the Congress. Even when it comes to basic research, and I have difficulty sometimes in communicating this to scientists, the Congress perceives that basic research is going to produce a concrete good, and it won't hurt if they had that perception at least about the social and behavioral sciences, even if you had to use it—that perception—to disguise your goal of getting good basic research, which was not really aimed at achieving some concrete result.

Let me give you a specific example, because it intrigued me in your testimony. Dr. Luce, you pointed out the fact that we're making much progress in linguistics. For example, we may get to the point where we can engage in computer translation or we can use natural language to program computers.

Dr. Smelser, you pointed out growing internationalization, which relates to that, in the sense it's going to require more ability to move across languages. We can support one center just focused on that almost, and we could justify it in many very practical ways, even though it might be just basic research, because we can see what it leads to. That's not a question, but you may comment.

Dr. **Simon**. Well, I think that is quite right. We should not expect any more than any group of scientists should expect that we get public support for research simply because it's fun and exciting for us to do. That's why we do it maybe, but that's no reason why society should have it done.

The potential bearing of social science research on our society and the way we live individually and collectively is so direct and so immense, maybe sometimes we take it for granted, and maybe sometimes we don't say enough about it, but this morning you just gave several examples of implications.

We mentioned several examples that have important long-term applications for education. The whole area today of expertism and artificial intelligence is much influenced by social science research, and I would agree, if we want support on the scale to which we would like to become accustomed, then we have to make our claim in terms of society's needs and what in the long run we can contribute to those needs. I think most of us share the faith natural scientists share, it's better to know than to be ignorant.

As we deepen our knowledge about any phenomenon, there are going to be important applications of that to human affairs. That's the faith that sustained fundamental research in the natural sciences. I don't see any reason why it should be different here.

Mr. **Brown**. We're fortunate in this country, there has always been a strong dedication to this principle of education and research in the sciences, but for the bulk of our history, we carried that out through funding of institutions of higher education on an across-the-nation basis, and there weren't separate programs for funding this esoteric research. It's these programs we are concerned with now as we see the vital importance to the Nation's progress and the difficulty of providing the funds through the normal channels that we used for so long.

I think it's our hope in this task force that we can create some greater awareness in the Congress of what these changes we have been discussing have been and how we need to adapt our mechanisms to handle them.

Mr. **Walgren**. Mr. Fuqua.

Mr. FUQUA. You mentioned in your testimony about the three criteria for good research, and one promised to be useful—maybe I am paraphrasing—is it always possible to do that in basic research, particularly in behavioral science?

Dr. SIMON. In basic research, you are trying to answer fundamental questions about how things happen, how things work. And as we know through the whole history of the sciences, including the social sciences, it's very difficult at that point to predict what difference it's going to make when you do know. Yet we have a very long history of finding that those applications do appear even out of such esoteric things as pure mathematics.

In the social sciences, there's no reason to suppose it's different. There's also no reason to suppose you can immediately predict what those applications are going to be. If you are studying out human beings learn, and if you find out something fundamental about that, it's hard to see how that can fail to have implications for the educational process or can fail to have implications for how people make decisions in business.

And so I think that it's very easy to describe specific areas that will be affected as that knowledge deepens, and, of course, it's very easy to describe specific areas where those impacts have, in fact, taken place. We complain, probably ought to complain, about the way our economy works from time to time, but it certainly works with fewer fits and starts as a result of what we do understand about economics than it would be if we were a bunch of barbarians and didn't understand anything about it. In fact, we probably couldn't make our institutions work without a large number of people in the society having a pretty good journeyman's understanding of economic principles.

Mr. FUQUA. How does basic research in behavioral science in universities differ from that of profit organizations or think tanks? Is there a different approach to that?

Dr. SIMON. Profit organizations can do research when there's a nearby application that causes somebody to want that research to be done. So it's always been true that the universities in our society have borne the main burden of doing the fundamental research that isn't immediately capturable in some kind of an economic enterprise. We can explain that in public goods terms, too. Who will foot the bill to find out something about the atom until you find out something that's almost patentable? Who will foot the bill to find out something about the basic ways in which the human mind works before you can turn that into a more profitable textbook or a more profitable visual aids system for education or something of the sort?

I think there's got to be the same division between the public and private sectors here that you find in any area of developing new knowledge, with the public sector having a particular responsibility for the basic end of it.

Mr. FUQUA. In many of the sciences, such as high energy physics, maybe astronomy and others, develop a set of priorities of what they think they need. Is that possible to do that in behavioral sciences, particularly for supporting Federal research?

Dr. SMELSER. It's been a subject on the mind of the committee all the way along as to the tightness with which we approach this notion of priority. We think, we have decided that there are two extremes that the committee could adopt in this regard. One would be a completely shapeless approach to things, say we're legitimate because we exist and ought to be supported. We rejected that as an irresponsible extreme for us to take.

The second would be to take the idea of priority very, very seriously and pretend that we could come out with a list of 10 to 20 areas and rank them and say this is it. We think that's also irresponsible. We think that this degree of flexibility and freedom has to be observed in identifying important areas, and really it was out of consideration to this sort that we are now undertaking on an inductive basis, by looking at these fields which appear to have the most ferment and most promise to come out with clusters of areas we identify as being most important without attempting to dictate this is what the future is going to be.

We don't think that would be in the spirit of scientific inquiry to try to be that definitive about priorities. However, we will undertake the obligation we feel to give some guidance to those agencies which are responsible for our support as to what in our best judgment constitutes promising areas, and those which would merit supplementary funding.

Mr. Fuqua. Dr. Luce.

Dr. Luce. If, as I think is going to happen, we come up with proposing some of these centers and larger activities, I believe that it will be possible to do some listing of priorities there, which is very much analogous to what the astronomers did; they talked about various kinds of centers and large equipment that they wanted, and they established a priority among those.

What I think we will not—I know we will not attempt to do and which I think some people might like us to do, but I think it's totally impractical, is to talk about priorities among the several social sciences. That's a very subtle, political problem. It's not different than the problem of saying what are the priorities between physics, chemistry, mathematics? That—I don't think you will find the natural sciences willing to confront that kind of a question head on, and we're not going to confront the analogous question in our collection of sciences.

Those decisions relative to allocation of moneys to various subfields are very hard for the sciences to deal with in a straight, head-on way. I think that it evolves through various kinds of political activities in the government, in the funding agencies, and that that probably isn't going to be enormously changed by our report, I don't think.

But we'll certainly look carefully at proposals about large incremental centers and the like and attempt to give some sense of what we think is most important of those.

Dr. Fuqua. The parent group of your committee, the National Research Council, was talking about supporting social science research, that the quality of work is highly variable and on the average relatively indistinguished. With the use of peer review and careful selection and so forth, is there anything that can be done about that, or do you hope to answer that in the committee that you cochair?

Dr. Luce. I think, in my opinion, that the statement is not accurate. I think it can start looking in detail at the kind of research that's being supported, at least in the areas I am familiar with, that's just not a very adequate description. I think the quality is a lot higher than that indicates. That doesn't mean there aren't some lousy pieces of research, and a few of them have gotten a lot of publicity in the course of time, but there is an enormous amount of sound research, and I think the peer review system is pretty good at spotting them. It's not a perfect device, and it's sometimes hard for certain kinds of activities to get proper peer review.

However you divide up the pie, anything that doesn't fit the divisions, well, is in a certain amount of trouble. We could tomorrow redivide the pie, and next year you would have things not fitting the new division. It's almost imperative to the nature of society we're going to have these kinds of problems.

But the fact remains, I think, that on the whole peer review, people are pretty responsible. They're taking their job seriously. The quality of the work is better than that statement would indicate. I don't remember who made that statement, but I would have to disagree with it.

Dr. Simon. Was that statement made in regard to the Rann activity a few years ago? The statement sounded a little familiar to me.

Mr. Fuqua. I think that was.

Dr. Simon. That was a committee of which I was chairman. I have to take responsibility for the statement. It was directed at a series of programs being supported in Rann, and those programs were essentially discontinued shortly thereafter, I wouldn't say as a result of, but at least in accordance with a sentiment expressed by that committee. That was not about all social science research.

Mr. Fuqua. I think, though, a hazard we all face in any research that's not of a pure science is the fact—and it's not an exact science—is that it can be subject to criticism, and that's where we run into problems in the Congress or public support for behavioral science, particularly in past years, I think that has subsided considerably in recent years. But that was a big concern of getting public support for behavioral science.

Thank you, Mr. Chairman.

Mr. Walgren. Thank you, Mr. Fuqua.

Mr. Slaughter.

Mr. Slaughter. You all have the handicap of so many different opinions on behavioral sciences. Most people don't have that many opinions on the physical sciences, or conclusions. I gather what you're saying, though, is that for the selection of research projects, you would follow basically the same procedures as followed in the physical sciences—peer review. No major changes?

Dr. Luce. No.

Mr. Smelser. I think the only qualification on that would be, at least in my own opinion, would be that the peer review, like any system, can become brittle, and we see some signs there are limitations, if the peer review system is by academic discipline. Peer review I regard as the most reliable or least unreliable system you can have, but the fact is there has to be a constant self-conscious examination of what is going on, and perhaps that can get built in over unexamined use of it.

Dr. **Simon**. I think, too, if the opportunities open up for the types of longitudinal, more institutionalized projects that were mentioned this morning that we're going to have to use a variety of procedures, just as in the natural sciences, the procedures by which we arrive at the decision we need a new national laboratory, a new major piece of equipment are not exactly the same as procedures whereby we give $100,000 to a researcher for 3 years, a couple of years' activity.

I think the review by one's peers is a part of a much larger and more complex process, and probably plays its largest role when you are talking about allocations to individual research, individual investigative research policies within an established framework rather than with these larger shifts in funding.

Mr. **Slaughter**. Thank you. I have no more questions, Mr. Chairman.

Mr. **Walgren**. Thank you, Mr. Slaughter.

If you had one wish, what would it be?

Dr. **Simon**. Are we allowed to say money?

Mr. **Walgren**. I was struck by the concept of the expected utilization as a basis of decisionmaking, and, of course, the Congress is no different than anyplace else, and so I am sure that in looking back on this whole area, we all sense how important the utilization or the expected utilization or ability to utilize of the research might be, and I suppose the social sciences in pursuit of research money shouldn't expect the Congress to operate on a different decision-making pattern than any others at that point.

But, again—let me ask one other thing. It seems to me that, particularly sensing the direction of my thoughts and others in the Congress, that we are mired in some kind of controversy and swing of support, and Dr. Simon, you said that taking a controversial area was somewhat like taking one issue or the tail of the elephant or one very—one point on the spectrum, there's this range of other research that's really going on that really should not be involved in that kind of judgment.

And yet I sense most of the ups and downs of the congressional process really coming because the political process is a pendulum, and by definition, the pendulum will swing out from under or in reaction to the solutions just proposed. Is there some way that we could and should insulate the rest of that spectrum from that swing of the political support process out from under the solutions and interest just recently proposed?

Dr. **Simon**. Well, I suppose that one of the major elements of importance of an institution like NSF, an institution like NIH, is that it provides a stable institutional environment within which an activity can go on over a period of years.

That doesn't mean that budgets aren't going to go up and budgets aren't going to go down, that emphases aren't going to shift. They should. I would expect that social sciences can get that same kind of stability as they are increasingly accepted as an intergral part of the activities of agencies like NSF and NIH.

The agency itself, NSF, has I think with a considerable enthusiasm accepted its broader responsibility for science, including social and behavioral science. In NIH we have had a more checkered history or NMIH in the last couple of years in which the rules of the game seem to have changed again, and that responsibility for support of a wide range of social sciences is now not being accepted as within their charter.

But I don't see any way you stabilize that beyond the longrun influence of more people understanding what it is all about and thinking it is useful. I don't see how you stabilize it beyond putting it within the decisionmaking powers of stable institutions of the Government. And for that reason, I think, for a large range of social sciences support the National Foundation, the Foundation has been a very inappropriate home.

Mr. BROWN. Dr. Simon, it seems to me that this is made to order for the application of what you call SEU theory, subjective expected utility, and if we could apply that in some realistic fashion, or establish the groundwork in which that utility perception is enhanced, it would do a great deal to facilitate enhanced funding in many different ways and not in the NSF, which primarily would support basic research.

A lot of what needs to be done is applied research. For example, in the Census Bureau, the Department of Agriculture, they used to have an excellent program which has been in decline for some years. A number of other agencies could have an enhanced program if that SEU function was somehow improved somewhat.

Dr. SIMON. Well, I think you have pointed out a fact before you can apply that decision. This is exactly what some of the research issues are now. You have to have ways of measuring what the utility is, of whatever it is you are deciding on, what the utility of social science research is. I don't think that gets precise measures in dollars anymore than the utility of high energy physic research lend itself to precise measurements in dollars. I think we form some kind of an assessment of the importance of the role it can apply in our society as a whole. We try to point to a specific institute where the application is fairly direct and fairly traceable, and we simply have to do the best we can to make those connections real and not make them believable. I guess we have to try harder.

Mr. WALGREN. Is there some potential in the cross-disciplinary organization's ability of the professions themselves—I am struck by this lack of connection between psychology and economics, and if you fund an economic department, you don't necessarily fund a psychological element—you fund the psychological department. You don't fund the economic element of it.

Is there some way that either it is happening where there is a unit forming that would enable us to bridge those separate disciplines? How can our funding system be caught up with that or catch up with that?

Dr. SMELSER. I will try to respond to that question.

First I will make a comment about the kind of culture that has come to pervade our own committee. We have a range of people on our own committee as broad as the whole spectrum of behavior and social sciences, and one of the apprehensions that some people felt would be that this would simply be a kind of trading game in which we would all sort of agree that we were all limited, and we would think of including everything.

Well, that has not happened. There has been a real sense of—as you put it, sort of coming together of the common institutes of the enterprises as a whole and an effort to really cut through those kinds of lines and not define our mission as a number of disciplines in competition with one another.

We have that obviously built into the whole organization of higher education. We have it built into the separate existence of professional societies and lobbists and so forth, and the people speaking on their discipline. In response to your question, I think our committee at least, and to some degree in the larger scene, this kind of competition is showing some signs of diminishing.

Dr. **Luce**. You will find that as disciplines, for example, parts of economics and psychology begin to have something to say to one another, that various procedures and developments take place. You will have concerns of an interdisciplinary sort. You may get the development at certain universities of degree programs that cut across departments.

You may get the development of research at a university that again cuts across departments. These are the kinds of mechanisms that within the university framework, at least, one used to try and foster these interdisciplinary developments, and in some cases when it goes on and proves to be successful, you get the creation of new disciplines out of that, and you may get over a period of decades you may get——

Mr. **Walgren**. Is NSF sensitive enough to respond to that kind of new entity?

Dr. **Simon**. Yes, I would maybe give a little more qualified answer to that one. It is organized by discipline now fairly strongly; that is there is an economic unit and a political science, sociology, et cetera, down through the various aspects of psychology.

At one time it had a special projects group which was particulary sensitive to emerging new areas, but I think any agency like NSF simply has to reexamine every few years when the boxes in which they put things are the right boxes.

Three examples—statistics has become a field with its own identity in my professional lifetime, and it took quite a while before it was recognized as something that needed funding under its own label. Cognitive science for some reason or another caught the attention and imagination rather rapidly, and I think now there is pretty good provision for funding things. That is a very interdisciplinary field funding things across those boundaries.

Economics and sociology are moving more volume, but at the present, I sense a lot of activity in that field in decision theory that brings in both the economic and the psychological aspects. So, we may expect agencies like NSF responding to that within a fairly short time, though I don't think they are really well institutionalized at the moment to take care of that one.

Dr. **Luce**. They are certainly not institutionalized. It is going to take probably ad hoc arrangements for a while to handle it. But I think Professor Simon is right—that happens to be one of the interfaces that is very lively, and you might be surprised to know that work on animal behavior, tradition, is entering into that interplay. There are results out of that literature that look as though they are quite pertinent to the problem, and there have been conferences.

There was one last June at Harvard where you had economists and psychologists of various sorts, including the animal behavior types talking to one another about the discoveries that have been made, so that it is handled at the moment largely by special conferences, special funding. It is certainly not an area in which you apply for a grant in NSF except to the extent that NSF, when it gets a grant proposal that doesn't fit the boxes, usually gives the proposal to two or more of the boxes to look at and then tries to sometimes come up with joint funding from divorces.

This is a less than perfect device, but it is what is often used as the field of first coming into view.

Mr. **Walgren**. Mr. Brown.

Mr. **Brown**. Let me ask Dr. Simon one question. How long has this subjective expected utility terminology been in existence, and the decisionmaking theory as a field?

Dr. **Simon**. It really began to jell about the World War II time, a little afterward. The work of Morganstern on the theories over games which they also axiomatized the utility business work of definity, and then was put into specialist polished form by mathematical statistics, and Jimmy Savage—that was one of Savage's books in the middle fifties.

Dr. **Luce**. Fifty-five.

Dr. **Simon**. I have a vague recollection that Keynes used to make some reference to this wild card SEU thing, although he called it consumer propensity to consume——

Mr. **Brown**. Is that the same general——

Dr. **Simon**. Well, it is a principle which one can derive from a general utility maximization principle by making some assumptions about how a consumer divides his budget between consumption expenditures and investment or savings.

The Keynesian theory was a generalization about how that division gets made. It is really a subset of this other.

Mr. **Brown**. I got the distinct impression he didn't figure out any way you could decide that.

Dr. **Simon**. There have been a lot of empirical work done on the basis of that one. Milton Friedman is famous for an attempt to show that the division between savings and consumption really doesn't change with level of income. I didn't say it quite right, but he did some basic empirical work on that.

Mr. **Brown**. By the way, we have come a long way since Keynes.

Dr. **Simon**. Yes; up hill and down.

Mr. **Walgren**. I think we are coming full circle.

Mr. **Slaughter**. Nothing further.

Mr. **Walgren**. On behalf of all of us we appreciate your views and the effort that you have made to come and be with us. We count on each of you as a continuing resource to us, and we hope that there will be a very positive outcome for all of this for the social and behaviorial sciences.

We will stand adjourned until tomorrow at 10 a.m.

[Whereupon, at 12:15.p.m., the task force was adjourned, to reconvene at 10 a.m., Wednesday, September 18, 1985.]

[Questions and answers for the record follow:]

Reply to Questions
Task Force on Science Policy Hearing
on The Role of the Social and Behavioral Sciences
September 17, 1985

Herbert A. Simon
Carnegie-Mellon University

1. Informed Use of Science and Technology

To what extent do the social sciences help the nation make informed use of the discoveries and technologies produced by the physical and life sciences?

Providing guidance for the application of science and technology is one of the most important functions of social science knowledge in our society. To cite a single example: The SST project was killed about 15 years ago on the bases of two analyses: an economic analysis that showed it would be a loser, and a meteorological analysis that showed it would deplete the ozone layer in the atmosphere. The economic analysis was correct, as the British-French experience has shown; the analysis of atmospheric chemistry proved later to have been wrong.

Several times (during the Johnson administration and at the present time), social scientists have been called upon to advise whether widespread unemployment is caused by, or aggravated by,rapid technological change. I cannot predict the outcome of the current study (by a panel convened by the Committee on Science, Engineering, and Public Policy of the NAS), but the previous committee was able to show that technological change was unrelated to long-term unemployment, and that employment levels would not be raised by curtailing the introduction of new technology.

Social psychologists and sociologists have contributed a great deal to our understanding of the short-run impacts of technological change, and of the policies that could be followed to shift short-run costs from individuals who are immediately effected by changes to the society. Today, a number of scientists in these disciplines are studying the impact of the computer on the nature of work and social interaction in factories, offices, and universities.

Among the major technologies that have transformed our society in my lifetime are the automobile and airplane, television, and air conditioning. Social science research by demographers, geographers, sociologists, psychologists, and economists have helped us to understand the many indirect, as well as direct, effects produced by the introduction of these technologies (e.g., the massive migrations of our population brought about by air conditioning), and, when heeded, have helped us respond to these effects (e.g., through the guidance they provide to land planning and transportation planning authorities).

Psychological studies of the impacts of television on viewers, while not nearly as extensive as the importance of the topic requires, are beginning to give us solid information on the effects of TV upon the behavior of children and adults.

In my own field of computer science, I have found myself in the dual role of producing some of the new technology as well as interpreting its impact from the standpoint of my previous research in organization theory. From the very beginnings, 35 years ago, of the introduction of the computer into business and society generally, businessmen and others have seen the importance of exploring the organizational consequences of this new automation, and I have found myself responding to almost weekly requests to show how social science knowledge of organizations can throw light on these questions. Within the past five years, with the introduction of personal computers and wide public awareness of "expert systems" and other developments in artificial intelligence, the interest in information about these matters has become even more intense, and has extended to popular magazines and newspapers, all of which are looking for informed opinion about the social implications of artificial intelligence. I could easily spend all of my time responding to such requests.

2. Social Science and Social Policy

To what extent has past social and behavioral science research contributed to the formulation of social policies?

An important example of social science that influenced public policy was Myrdal's *An American Dilemma*, which, by holding up a mirror to ourselves, had a major impact on policies of racial integration. This study, in turn, was informed by a whole generation of research, not just upon race, but upon interpersonal and intergroup relations of all kinds.

Increasingly, our society must adjudicate among competing claims to resources -- for example, it must reconcile our needs for energy with equally pressing needs to protect the environment. Economics has often been useful in providing a framework for analysing these problems, and in constructing order-of-magnitude estimates of the social costs and values at stake. For example, economists played an important role in the work of a committee I chaired a decade ago, advising Senator Muskie's committee on the reasonableness of current levels of air quality and auto emission standards. The costs of controls had to be compared with the social benefits of improved air quality.

Similarly, some years ago, economists, collaborating with statisticians and sociologists, undertook studies of the effects of the proposed negative income tax, a measure aimed at improving our public assistance policies. Unfortunately, their ingenious and informative experiments were halted too soon by budget cuts, but not before they had cast much light on the question of whether incentives to work would be reduced by a negative income tax. (It wouldn't be reduced very much, they concluded.) Here is an area where a revival of the research program would be of great value in informing public policy on a yet unsettled issue.

Psychological research on the causes of and remedies for mental illness has provided important information for decisions on care of the mentally ill. The reappearance of "street people" in the United States points to the importance of new research to understand who these people are, and what the alternative policies are for more humane and constructive treatment of them.

The testing instruments developed by psychologists (whatever their limitations and our questions about them) have provided a base of information, fallible but much better than ignorance or hear-say, about the trends in our educational institutions and the levels of achievement of students. Research on the reading process, a very active area in cognitive and educational psychology, has had a major impact on methods of teaching reading in the schools, and on social programs to deal with illiteracy.

Criminology has been an active area of inquiry in sociology for a century or more. It has provided a steadily deepening understanding of the nature, causes, and consequences of criminal behavior that continues to inform policy making in this difficult area. As almost random examples of recent important work, studies of the effects of changes in the size and constitution of juries should be mentioned, as well as the periodic surveys of "victimizaton," which give us much more reliable statistics on the frequency of crime than we have ever had before.

Economic analyses have been important in anticipating the consequences of deregulation in such industries as air transportation. Economics also plays a major role, of course, in discussions of national economic policies. Our disappointment in the failure of economics, so far, to solve completely the problem of stagflation, and the frequent disagreements of prominent economists on policy issues should not blind us to the major positive contributions of economics. There was almost complete agreement in the profession, for example, that a policy of reduced spending and high interest rates pursued by the Federal government would both reduce inflation and increase unemployment -- which it did.

The disagreements of economists that lead to their prescribing different public policies is in considerable part a disagreement as to facts, and in particular, as to how people form expectations about an uncertain future and use those expectations to make spending and investment decisions. If we are prepared to support major research on economics at the "microlevel," aimed at ascertaining how people do cope with uncertainty, we can begin to reduce the economics profession's own uncertainties about these key aspects of the economic system. This is a kind of research that is almost unknown to economists (who are accustomed to securing their "facts" by sophisticated manipulation of public, aggregative statistics), and getting it done will require not only substantial funding but also considerable reorganization within the profession of economics itself, and major change in the training of its graduate students.

3. National Priority Setting

What contributions to national priority setting should the social sciences be making that they are not making now?

We must be careful in speaking of social science contributions to national priority setting. Priorities are determined by the weights attached to alternative values, while the task of the social sciences is not to impose values but to explore what courses of action will realize alternative sets of values, and what consequences will follow from pursuing particular values. Within a democratic society, science, social or natural, can advise on means and consequences; science itself has no special competence for selecting the final goals at which a society should aim. That is the task of political and social processes.

Within its limited competence, the social sciences can perform a vital function by directing our society's attention to problems that need that attention. One such area (in which I am not expert, but where I sense a considerable confusion) is national immigration policy. There are many conflicts of value here, which social science cannot resolve. But there is also great uncertainty as to the consequences for our Nation and our neighbors that will flow from one policy or another.

I mentioned earlier the need to reexamine our public assistance and other welfare policies. Here, a vigorous program of research, including experiments in the field like the negative income studies, could play a valuable role in setting national priorities for welfare by giving us solid information about how people respond to particular forms of public assistance.

We continue to hope for much more from our system of public education than the system can now deliver to us. At the same time, we are in the midst of a scientific revolution in cognitive science that is giving us a whole new insight into the nature of learning, and especially, learning with understanding as distinct from rote learning. Cognitive science research is beginning to be able to illuminate educational policy in a way that psychological research in the past was not prepared to do.

Of course there is no goal in our society today more important than achieving national security with peace. No route to that goal will be easy, but making anything less than an all-out effort to attain it is unacceptable. The social sciences know a great deal about conflict and conflict resolution, and can learn a great deal more. Even the knowledge we have today does not seem to be put to much use by those responsible for national security policies, and peace research is poorly funded. I would put this area at the very top of my agenda, without expecting miracles but with a recognition that even modest contributions to the sanity with which we handle these matters may be of the utmost importance to peace -- that is, to our survival.

4. Government Role in Facilitating Applications

What is the role of the Government in facilitating or inhibiting the contributions of the social sciences to the resolution of issues of national importance?

The first and most obvious role of the Government is to fund basic research that can provide the sound knowledge from which all applications to society must derive.

A second role is to provide, in each of the Departments to which social science knowledge is relevant, adequate staff groups, themselves knowledgeable about the social sciences, who will be aware of knowledge that is important to the work of the agency and who will help to bring it into the administrative process. Every consultant to government or industry knows that you cannot successfully advise or inform an organization that does not have within its boundaries people competent to receive and translate the advice. The present picture in the National government, with respect to such internal groups, is very uneven. The Treasury Department, for example, has a long tradition of economic research, and is well able to ingest new knowledge and developments in economics. At the other extreme, most observers view the State Department as having a very weak capability to use social science knowledge.

The support of mission-oriented (in the broad sense) research is another way in which agencies can enhance their ability to use social science findings, knowledge, and techniques. In both natural and social science, the Department of Defense has an excellent track record in this respect. In the social sciences, for example, it has long supported psychological research of wide relevance to its responsibilities for selecting and training military personnel.

The Congress itself must be equipped to obtain, evaluate, and use social science knowledge. Congressional staffs, the Office of Technology Assessment, the reference services of the Library of Congress, and the use of the advisory services available from the National Academy of Sciences are examples of Congressional responses to the need for information from experts. I would not presume, without study, to suggest how these instruments can be strengthened to make better use of social science knowledge.

Beyond these specific routes, the main role of Government in facilitating the contributions of the social sciences is to make certain that the First Amendment is well enforced. In a democracy, social science knowledge, to be useful, must enter many millions of minds, through lectures and books, and filtered through the popular media. A healthy academic community and healthy channels of communication are the main means of knowledge diffusion. And the very institutions of government must be open, as they generally now are, to the scrutiny, not only of an "investigative" press, but also of scientists whose task it is to describe and interpret those institutions to the society.

5. Budget Processes

To what extent should Congressional support distinguish among the individual disciplines within the field of social and behavioral sciences

There is no simple answer to this question. The social sciences are, as observed, very diverse. Organizational structure within the National Science Foundation already establishes some separation between experimental and physiological psychology, on the one hand, and the remaining social sciences, on the other. However, some of the most exciting and promising research areas in the social and behavioral sciences at the present time cut squarely across the disciplinary boundaries. Cognitive science is one such area -- embracing cognitive psychology and much of linguistics, but also some parts of anthropology and philosophy and history of science, as well as non-social-science disciplines like artificial intelligence. Another such area is the theory of decision making, where important work is going on in economics, experimental psychology, political science, and statistics.

At least within the National Science Foundation, there is much to be said for leaving budgetary boundaries flexible at the present time, and not freezing them at a moment when the disciplines themselves are in flux. Much of the most important work lies across these boundaries, and will not thrive if the disciplines have too complete a control of the full purse. Funding agencies must be encouraged, of course, to identify the growing edges, whether inside or outside the disciplines, and to provide appropriate mechanisms for supporting them.

Perhaps more important than allocating the presently available funds is creating expectations about funding possibilities that will lead social scientists to think seriously what research tasks they would undertake if the resources available were ten times greater than they are now. Measured against current Federal budget stringencies, that is, of course, a Quixotic view. Measured against the need for expanded social science knowledge as a foundation for addressing vital national problems, it is a view of utmost realism.

Responses of R. Duncan Luce to Questions
Raised by the Task Force on Science Policy
February 18, 1986

1. Q. Certain criteria may apply generally to the Federal Government's support of science. In making decisions about the support of the Social and Behavioral Sciences by the Federal Government, what criteria should apply?

A. The major reasons for the Federal Government to support science are the following: (i) to foster developments that generally strengthen the country, including its economic and technological base and national defense, (ii) to satisfy various national aspirations relative to scientific leadership in the pursuit of basic knowledge, including a number of large projects (e.g., space telescopes, high energy physics) that would not otherwise be pursued, and (iii) to help maintain the system of first-class universities that pursue much of the best research in the country.

So far as I can see, there should be no difference in the criteria that apply to the natural and social sciences, although, quite obviously, the several sciences will realize these criteria in quite different ways. For example, under (i) the social sciences play a special role in providing a great deal of the data and analyses concerning policy issues. In particular, they are deeply involved in creating and bringing to bear economic, social, and political data bases on various issues of great national concern. Under (ii), the main large scale projects are various forms of long term, data collection centers. This will continue to be of great importance, and I think the evidence suggests that some additional ones are warranted if we wish to obtain the data needed to make better decisions about such problems as crime. The issues of maintaining first rate universities as a unique national asset through the support of their research is in no way different for the social and behavioral sciences and the natural sciences. Federal funding of carefully selected projects will certainly strengthen the major research universities, creating centers of intellectual activity and discovery that are the envy of the rest of the world. It is also important to realize that at the present time there is a great deal of work going on at the interface of both engineering and biology and behavioral science, most of which takes place at the universities.

If the question addresses the criteria to be applied to more specific research projects, then it must be pointed out that somewhat different criteria pertain depending on whether the research is primarily basic -- aimed at understanding underlying principles -- or applied -- aimed at informing policy matters. As I know much less about the latter, I restrict myself to the former. For basic work, two criteria are essential. First is whether or not the issues being investigated are of scientific significance and bear on some fundamental question whose answer could make a difference to the further development of the science. Second is whether or not the research plan is methodologically sound in the sense that it appears suited to answering the questions raised and is free from all sorts of subtle errors, ranging from experimental design, procedures for handling subjects, to statistical analyses. In practice, there is some tendency to slight the first criterion, about which it is sometimes difficult to obtain consensus, in favor of the second, which is often more amenable to discussion and can be sufficiently complex in the social sciences that it sustains the interest of both scientific peers and the staff members of granting agencies. On subjects for which there is substantial theory and a large base of cumulative empirical work, consensus about scientific significance tends to be greater than in areas in which the work is more speculative or fragmentary.

2. Q. It is widely recognized that many of the changes now occurring in society are attributable to the advances taking place in the physical and life sciences. To what extent do the social sciences help the nation make informed use of the discoveries and technologies produced by the physical and life sciences?

A. (See also the answer to question 4.) I am aware of two ways in which the behavioral and social sciences manage to help inform the use of physical and biological discussion and resulting technologies. The most important is to provide some insight into how and why people will react to and shape the changes involved. Often, especially when cultures foreign to our own are involved, reactions to innovations are very different in important ways from those anticipated by engineers and physical and biological scientists. Sometimes the unexpected response could have been anticipated, at least in part, had certain relevant social scientists been consulted. This is not to say that we can or soon will be able to work out the full consequences of any given technology such as the introduction of television or of personal computers, but certain predictions are possible and sometimes they differ from one's intuition.

A good example of a technology in the making that draws on all three areas -- physical, biological, and social science -- is biotechnology. It clearly rests on the modern developments in biology and microelectronics, but it also depends upon research about long-term markets for various potential products. Moreover, to the degree risks are involved and public acceptance or rejection of the technology is an issue, understanding risk perception and the formation of public opinion is not only relevant, but often financially vital.

Given the crucial importance of anticipating social reactions to technological change, it is quite surprising how little investment the government has made in research on the social causes and consequences of such changes.

The other way the behavioral and social sciences inform the other sciences is by suggesting new technical solutions. The fact that our visual and auditory perception, speech production, and memory are vastly more flexible and organized than current technology is capable of mimicking offers the possibility that as we understand how people do these things then new ideas for physical technology will arise. Several examples of such technical transfer from behavior to engineering are now in the process of development, such as efficient transmission of video signals over telephone lines, which takes into account what information matters to the human visual system, and photography in which the inherent colors of the scene can be separated from the light that happened to be impinging on it at the time the photograph was taken.

3. Q. An important criterion for support of research in the social sciences must necessarily be the expectation that the resulting findings will contribute to the formulation of social policies, the solution of social problems, or the decision-making process at the judicial or regulatory level. To what extent has past social and behavioral science research in any of the disciplines contributed to the formulation of social and other policies, and what are the prospects for the future?

A. I assume that this question is really confined to applied and not basic research (see answer to question 1).

It is often overlooked just how deeply the social sciences are already involved in all sorts of policy matters. Almost all policy makers have been exposed to a good deal of fundamental social science knowledge during college and have absorbed a huge amount of theory and fact -- often forgetting where their "common sense knowledge" of many aspects of the world first began. At a more concrete level, each of the social science disciplines have been thoroughly involved in the setting up and development of many of the now familiar data bases which are the source of almost all of our statistical knowledge about our society and government. Moreover, almost all analyses carried out on these sources of data are the products of social science research. Various methods of predicting and evaluating the outcomes of policy proposals are due to social scientists. Among the methods used are linear and non-linear programming, systems of structural equations, econometric models, regression models, cost-benefit analyses and the like. A less explicit, but no less important, function of the behavioral and social sciences to policy makers is to explore and delineate issues that may be outside the knowledge of other types of scientists -- issues having to do with cultural differences, with various perceptions (e.g., risk perceptions), with organizational and human factors issues in large scale sociotechnical systems, and so on.

I see no reason not to expect all of these roles to continue into the future. The main problem is to find efficient ways to make policy makers familiar with the available sources of advice. To some extent, organizations such as the National Academy of Sciences serve knowledge brokering roles for major policy issues and a number of the private consulting firms do so for somewhat more modest ones. Both can be expected to continue these functions.

4. Q. The deeper understanding of human needs and social processes which is expected to be achieved from research in the social sciences should provide an enhanced level of understanding relevant to the national policy making and priority setting in many areas. What contributions to national priority setting should the social sciences be making that they are not making now?

A. In setting social priorities, the social sciences should serve an important role in attempting to work out the social and political consequences of social alternatives. In the past, they have done so sometimes through the use of existing information in various data bases and sometimes by means of various kinds of quasi-experiments. Over and over, we have found that the outcomes we expect by common sense either do not happen reliably or are so ill defined that it is necessary to do research to make explicit predictions. The social world is really more complex than most of us have time to master, and though we muddle through on the basis of experience often we can do better by applying systematic analysis.

To some degree, the social sciences currently play such a role, especially in the areas of economic and foreign policy. They do not play much of a role in science or defense policy. For example, in developing the NSF policies concerning the use of supercomputers, their voice and needs were little heard. They do not seem to have been much drawn into considerations underlying the Strategic Defense Initiative.

One major priority issue is the relation between investment in fundamental research and the problem of more directed work. Detailed studies of the history of research investment patterns in various societies and times should shed light on this ever troublesome issue.

5. Q. Research in the social sciences has advanced steadily in the decades since 1945, and many findings have emerged. The use or lack of use of these findings is not, however, automatic, and may or may not be justified in particular circumstances. What is the role of the Government in facilitating or inhibiting the contributions of the social sciences to the resolution of issues of national importance?

A. I assume that national goals are always best achieved by bringing to bear as much systematic knowledge as is feasible given the time and financial constraints involved. We also have a constitutional tradition of freedom to enter into public discussion. Therefore, I can see no real role for the Government ever to inhibit contributions from the social sciences to the resolution of issues.

As to facilitating, I think that there is a continuous challenge of educating new public officials about the growing range of things that the social sciences know and know how to do. I suspect many officials have limited knowledge about the data bases available, the methods for extracting and analyzing information, and the various ways in which they can draw on the social science research community. Let me amplify on the latter point.

At the present time, some use of the social sciences is made through various special programs for senior government officials, such as that at the Kennedy School at Harvard, and through the "knowledge brokers" that were mentioned previously. What is still lacking, however, is their adequate involvement in the forums --the various panels, boards, commissions, and the like -- that are used to develop recommendations about high policy decisions.

6. Q. In making judgments about the budgetary levels of support for the social sciences, the Congress has studied the proposed budgets for the individual disciplines, such as anthropology, economics and psychology. But it has customarily authorized and appropriated funding in the aggregate, encompassing all of these disciplines in one lump sum. The diversity of the individual social science disciplines and their differing degrees of maturity and relevance to societal problem solving might suggest that the Congress differentiate between the levels of support to be provided. On the other hand, the ability to make such judgments, and the time required to delve into the state and opportunities in each field, might mitigate against such an approach. To what extent should Congressional support distinguish among the individual disciplines within the field of social and behavioral sciences?

A. While it is true that much of the behavioral and social science work is conducted in the guise of rather sharply different disciplines and it also the case that many professional organizations, university departments, and funding agencies use these disciplinary boundaries in organizing themselves, few problems of major social or scientific significance are so neatly divided. As the Committee on Basic Research in the Behavioral Social Sciences (National Research Council) has been developing its "Ten Year Outlook" report, we have been greatly impressed by the strongly interdisciplinary nature of so much of the most exciting current research. Many researchers feel the disciplinary boundaries are barriers to be overcome, not fixed truths to be reified. So, it strikes me that for the Congress to make more detailed budgetary decisions of this kind would probably prove unhelpful to research, especially since it could seriously compromise the flexibility of the funding agencies to construct new categories aimed at facilitating cross-disciplinary activities. To foreclose or make far more difficult such arrangements would be a mistake. And it is unlikely that members of Congress will want to be kept so closely informed about rapidly changing research opportunities that they could be as responsive to these as are the full-time scientists running NSF, NIMH, ONR, etc.

Rather than attempt relatively detailed micromanagment of the funding of science, I would urge the Congress to seek out specific instances of new directions that seem to be slighted, ones for which a new program focus could be authorized for a fixed period and specific reports be filed. This is a matter of assuring adequate support for innovations that seem of particular importance, but it is certainly not a matter of trying to choose among disciplines.

Responses of Neil J. Smelser to Questions Raised by the Task Force on Science Policy

March 12, 1986

1. Q. Certain criteria may apply generally to the Federal Government's support of science. In making decisions about the support of the Social and Behavioral Sciences by the Federal Government, what criteria should apply.

A. The first criterion that should apply to the Federal Government's support of the behavioral and social sciences is that that research should be carried out at the highest possible level of scientific competence. This criterion applies to both basic and applied research; if either kind of research is not conducted with strong scientific judgment, its potential usefulness to those decision-makers and others who use that knowledge will be diminished.

The second criterion that should be kept in mind is that there should be a mix between (1) basic research, which permits those who do research in the behavioral and social sciences to be driven by the results of other theory and research, without reference to applications and (2) mission-oriented, or applied research, which is directed toward the understanding of specific problems toward which legislation and/or policy decisions might be directed. A healthy proportion of the former type of research is essential because: first, much of scientific advance is realized through the free inquiry of scientists following their own leads; and second, many unsuspected practical applications are derived from basic research that is directed to no particular practical solutions of problems.

2. Q. It is widely recognized that many of the changes now occurring in society are attributable to the advances taking place in the physical and life sciences. To what extent do the social sciences help the nation make informed use of the discoveries and technologies produced by the physical and life sciences?

A. There are two ways in which behavioral and social sciences may supplement and make informed and productive use of the physical and life sciences. The first is that many of the models and quantitative techniques developed in the physical and life sciences may find very productive use in developing behavioral- and social-sciences theory and investigations. There may be a reverse flow, too; mathematical and epidemiological models developed in the behavioral and social sciences may have applications in the physical and life sciences. The second is that the behavioral and social sciences can contribute directly and profitably toward understanding and predicting the consequences of the applications of knowledge that has been generated by the physical and life sciences. As we all know, the application of knowledge often is controversial; the establishment of nuclear power plants, the utilization of new medical technologies, and the determination of acceptable levels of technological risk are examples. All of these instances have a fundamental component of established knowledge in the physical and life sciences; but the development and application of the appropriate technologies are influenced strongly by cultural and social factors. For this reason the knowledge of behavioral and social sciences must be brought to bear on these issues.

3. Q. The deeper understanding of human needs and social processes which is expected to be achieved from research in the social sciences should provide an enhanced level of understanding relevant to the national policy making and priority setting in many areas. What contributions to national priority setting should the social sciences be making that they are not making now?

A. There seems to be two ways in which behavioral and social sciences can contribute productively to the setting of national goals and national policies. First, through the gathering, analysis, and interpretation of social data--for example, on spending patterns, public attitudes, crime rates and patterns--behavioral and social scientists provide a great amount of empirical data and knowledge that can be used productively by policy makers in the decision-making process. Second, the behavioral and social sciences can contribute to a knowledge and understanding of the costs and possible consequences of priorities (for example, massive increases in defense spending, massive support of primary and secondary education), without insisting that knowledge and understanding be the sole guiding criterion in the public choice of one priority over another.

4. Q. Research in the social sciences has advanced steadily in the decades since 1945, and many findings have emerged. The use or lack of use of these findings is not, however, automatic, and may or may not be justified in particular circumstances. What is the role of the Government in facilitating or inhibiting the contributions of the social sciences to the resolution of issues of national importance?

A. Three lines of improving the usefulness of social-science findings might be suggested. The first is to make a more systematic effort to include behavioral and social scientists on investigatory commissions and in standing agencies (for example, the Office of Science and Technology) where their expertise is evidently called for. Second, greater efforts should be made to identify and establish contact with those who have been responsible for generating potentially useful findings, and seeking periodic advice on the usefulness of those findings. Useful findings derive from the development, maintenance, and improvement of high-quality data bases. Third, those agencies (Bureau of the Census, Department of Labor, Department of Education) that themselves generate social data in the course of their operations could improve these data by expanding their reliance on advice of methodologically-sophisticated scholars in the relevant areas on how best to generate, code, and analyze such data. Those agencies might also make further efforts to make the data they gather more research-oriented to a degree, and more readily accessible to scientific researchers.

5. Q. In making judgments about the budgetary levels of support for the social sciences, the Congress has studied the proposed budgets for the individual disciplines, such as anthropology, economics, and psychology. But it has customarily authorized and appropriated funding in the aggregate, encompassing all of these disciplines in one lump sum. The diversity of the individual social sciences disciplines and their differing degrees of maturity and relevance to societal problem solving might suggest that the Congress differentiate between the levels of support to be provided. On the other hand, the ability to make such

judgments, and the time required to delve into the state and opportunities in each field, might mitigate against such an approach. To what extent should Congressional support distinguish among the individual disciplines within the field of social and behavioral sciences?

A. Insofar as decisions involving differential allocations to behavioral- and social-science disciplines (sociology, anthropology, economics, political science, etc.) are concerned, it would seem a too-detailed enterprise for Congress to develop detailed discriminations along these lines. Initiative for such discriminations would best be left to granting agencies--in consultation, it is hoped, with representatives of disciplines. What is of greater concern, however, is that agencies maintain sufficient flexibility in the funding structures and policies so that new ideas and lines of research that do not necessarily fall into disciplinary categories can be reacted to and supported creatively by granting agencies. The disciplines are not entities that are fixed forever; fresh ideas come within disciplines, among disciplines, and out of the blue. The mission of granting agencies should include a high probability that ideas and lines of research from all these sources be recognized.

THE ROLE OF THE BEHAVIORAL AND SOCIAL SCIENCES

WEDNESDAY, SEPTEMBER 18, 1985

House of Representatives,
Committee on Science and Technology,
Task Force on Science Policy,
Washington, DC.

The Task Force on Science Policy met, pursuant to call, at 10:15 a.m., in room 2318, Rayburn House Office Building, Hon. Doug Walgren (acting chairman of the Task Force on Science Policy) presiding.

Mr. **Walgren**. Let me apologize to everyone for being late. I did not anticipate one call I had to deal with.

In any event, the Task Force on Science Policy welcomes our witnesses today and we appreciate your coming.

Today we will focus on the use of the social and behavioral sciences, this being a second of three days of hearings that we will have, and we have a number of distinguished witnesses who have directly applied concepts from the social and behavioral sciences to industry and government.

Yesterday the task force was given an overview of the social and behavioral sciences, and tomorrow we are going to try to look at the role played by private foundations in supporting research in this area as well as a number of other concepts that tomorrow's witnesses will have contact with.

But today we have Dr. James R. Coleman of the department of sociology of the University of Chicago—and we are very happy to have you, Doctor; welcome to our proceedings—along with Dr. Newhouse of the Rand Corp. and Dr. Clark Abt of Abt Associates and Dr. Albers who is involved with social analysis with General Motors Research Laboratory.

We appreciate your making yourselves available to the committee. We have one other doctor, Dr. Douglas Bray, who is chairman of Development Dimensions International Corp.

We would like to go forward as a panel, if you would, and suspend our own discussion until after you have all given us some food for thought, and at that point I believe we can have a discussion in which we can all join in and see if we can emphasize or develop certain aspects.

Would any of the other members like to make any opening comments or give any thoughts at this point?

[No response.]

With that then, Dr. Coleman, welcome to our group and we are glad to hear you.

STATEMENT OF DR. JAMES R. COLEMAN, DEPARTMENT OF SOCIOLOGY, UNIVERSITY OF CHICAGO, IL

Dr. Coleman. Thank you, Congressman Walgren.

Should I turn this on?

Mr. Walgren. Yes. The mike, please do bring it in relatively close because I never know when the corners of the room can hear, and it needs to be relatively close to you so it picks up directly.

Dr. Coleman. Thank you.

Thank you and my thanks to the other task force committee members who are here.

What I would like to do is not to comment on social science itself or on what Government can do for the disciplines of social science, but rather the reverse question, the question of what social science research does and can do for the Government and for society.

Now, when I say that, I expect some persons would immediately exclaim that social science research doesn't do anything for society but does a lot to society. It does such things as bringing about busing, it's responsible for releasing criminals before they should be released and does a number of other bad things.

But I would like to make a comment in response to that and that is that the results of social science may favor one side or favor another side in a conflict. For example, recent research of my own on public and private schools which showed that Catholic schools produce higher achievement in verbal and mathematical skills than do public schools were very roundly denounced by many of the same persons who earlier had been quite happy to use the results that I found within equality of educational opportunity.

So that, as I said, the results may favor one side and may favor another.

To take another example, the negative income tax experiment which was initiated some years ago resulted in some analysis by sociologists which showed that such a program would sharply increase the divorce rates and reduce the remarriage rate. When this evidence came out in Senate hearings, that was perhaps a decisive influence in killing negative income tax legislation that was proposed during the Carter administration.

The question: Was that a good thing or a bad thing? I think arguments can be made on both sides, but no one, I think, would dispute the fact that it's better to know the consequences of a negative income tax program for the family before such a policy is initiated rather than not to know.

In this respect, I think social science is similar to natural science. The knowledge that is produced by the research is distinct from the use to which the knowledge is put, and I don't think there are many people who would say we should opt for less knowledge.

Now, this illustrates a first point I would like to make. Namely, that social policy research has become over the past 20 years a very important element in social policy. There is a good example, a good natural experiment that illustrates that, and that is in 1980 after the Reagan administration came into office or in 1981 there were sharp reductions in the budgets for basic research at NSF, but there was much less reduction in social science contract research that were called for in RFP's by Federal agencies. The reason I think is that the agencies didn't find it in their interest to eliminate this research. So, I think the question is not whether there will or will not be social policy research nor even how much of that research there will be. The question is how it will be done. In effect, I think what we are doing today is discussing the emergence of a new kind of information-generating activity as an adjunct to Government. So that policies that I think are made as a consequence of this task force's activities about how the activity, about how this new information-generating activity, is carried out will have consequences, I believe, for the way in which other policies, that extend far into the future, get made, and for the quality of those policies.

I think the crucial questions are three questions. One is how to ensure that social policy research helps rather than hurts the democratic pluralism of the policymaking process. Second, how to ensure that social policy research is done with sufficient independence to be objective, yet at the same time with sufficient responsiveness to provide relevant information when it's needed. The third is how to ensure that the individual skills and organizational capabilities to do the work are available and will continue to be available. I would like to comment briefly on each of these three points.

The first point which I regard as extremely important is the point of strengthening democratic pluralism. I think the first point that is important to recognize is that social policy research is not value free. The questions that are posed by that research and the kind of information that is sought in the research are predicated on certain goals and interests. It's of central importance that social policy research obtain information on these questions in an objective and unbiased fashion, but the questions themselves are not value free.

The problem then is ensuring that the information obtained is relevant to the interests of all parties who are affected in a given policy area. It should not be merely what the social scientist carrying out the research thinks is important. It should not be his values. It should not be merely what the officials in the Government agency believe to be important. What it should be is information that is relevant to the interests of all parties who are likely to be affected, and I think you will recognize that this isn't a trivial test. I would like to remind everyone here that the hearings before congressional committees are an institution that was not called for in the Constitution but developed early in the history of this country, directed very much to the same purpose. But that institution, that is, hearings before congressional committees, is predicated upon the assumption that all parties do have information relevant to their interests and that assumption, although it may have been well meant in the simple society that we lived in two centuries ago, is not very well met in today's society.

Now, the principle of making, of obtaining information that is relevant to the interests of all parties may be implemented in a number of ways. One is to require that in RFP's and the resulting contracts for social policy research there be a preresearch assessment of information needs of interested parties, with the results of that assessment to be used in a design of the research. Another device would be to provide for dual analyses, by different sets of researchers, of data that bear on a given policy, and open availability of the data to other researchers. There are other possibilities, but each of them to be effective has to be, I think, very carefully designed.

The important point is that information becomes an increasingly valuable commodity in modern society and citizens lose a portion of their rights as citizens if their information rights are not adequately protected.

An implication of pluralism that I suggested in design of and access to social policy research is that the results will be used not merely by the Government body that contracted for it, but by a variety of parties. Most of those parties will, in fact, be outside Government. In my experience, in fact, social policy research results are more useful to those without authority than those with the authority to make policy, and they are not always used in a way that neither those in authority nor the researchers would choose as some of the examples I have given have suggested. The end result of all this is not a more rational decisionmaking process by a Government agency. The result is, I think, rather an upgrading of the knowledge base on which policy issues are fought, and perhaps even an increase in the likelihood of conflict over policy, as more consequences of that policy come to be known. But the result is also an increase in the quality of the ultimate policy that results.

Now I would like to turn to the second of three points which has to do with independence and responsiveness. It's necessary to avoid two extremes: one is a research organization that is wholly dependent on a Government agency; the other extreme might be illustrated by university professors who in a university setting are too independent—they are unlikely to provide research results on time, and when the results do arrive, they are likely to be on a different topic. Both of those qualities are very good for basic research, but they are not very good for research relevant to policy.

What is necessary is that delicate balance of independence and responsiveness, and that's true both for the research organization and for the staff of the organization. The organization will necessarily be supported largely by contract funds for a specific project, but it won't be wholly—they should not be wholly supported in that way. There should be some longer term and more secure funding. There are examples of this in the natural sciences that arose in the Second World War. The Jet Propulsion Laboratory at CalTech, Argonne Lab at the University of Chicago, Lincoln Lab at MIT, APL at Johns Hopkins and several others. There have been some small-scale R&D centers in various areas in social sciences established in recent years, for example, in education and I think this is a policy which should be expanded.

There is another institution that I think might be usefully considered and that would involve initiation by Congress itself of social policy research. You ask why that might be so. I am saying partly in place of, partly in addition to that which is initiated by the executive branch agencies. You might ask why I think that might be a good idea. I think I'll mention just two points. One is since policy is, in fact, made in Congress, then Congress is a natural locus for research of this sort. The second point is that what might be described as the multiheaded character of Congress, which for some purposes might be regarded as a defect, is, I think, for this purpose, an asset because it can then ensure that pluralism of social policy research design as it's designed and carried out.

If this avenue were followed, I think the best mechanism would be through an expansion of the role of the Congressional Budget Office which Alice Rivlin brought into being some years ago. If this were done, Congress through CBO would contract for, and oversee the execution of, the social policy research in much the same way that executive branch agencies currently do. The evaluation of social programs initiated at the Federal level by congressional legislation would be contracted out and overseen by CBO. There are pros and cons to this, but I think it might be considered as part of an institutional mix to produce and make use of social policy research information.

My third point is the development of organizational capabilities and individual skills. There do exist now a few organizations that are capable of carrying out social policy research that involves large-scale data collection. Their staffs include many people who have learned wholly on the job. The data analysts are usually sociologists, statisticians, or economists, but in none of those disciplines nor any other discipline in universities does the appropriate mix of training for these policy research jobs exist. It's also the case that the project officers in the Government agencies are often inappropriately trained, the project officers who monitor and interact with the projects that are contracted for by the agencies.

The point is there are no applied science schools in social science, yet these constitute the appropriate model. In their absence, graduate training fellowship programs which would involve course work in hands-on training in the few policy research organizations that exist can be a substitute. But without some kind of vigorous movement in this direction there won't be a mention of moving toward the efficient frontier of having lower costs and more effective social policy research.

Now, what I am suggesting is that there must be some investment in maintaining the organizational capital and in training programs in order that the work be of higher quality and lower cost than currently is the case.

I haven't said anything about Government support of basic research in the social science for its own sake, and I won't do so because my purpose here is to report on the existing and potential contributions of social science research to society. But the neglect of the basic social science research, I think, will result in serious long-term costs to society. Let me give an example of this.

This is a book that was just published. Some of you may have seen reviews of it recently. There was one a week ago Sunday in the New York Times. The book is entitled *Crime and Human Nature,* by a political scientist, James Q. Wilson, and a psychologist, Richard Herrnstein. Wilson and Herrnstein did not carry out any research on crime for a Government agency, nor did they do any original research of their own. What they have done is bring to bear the results of a wide range of research, mainly basic research, on a single question, the question of what kinds of persons become criminals.

Now, they arrived at conclusions, I think, that have quite powerful policy implications. None of their conclusions are merely commonsense, and they could not have been drawn without a vast background of basic research as well as some policy research that was developed over the past 30 years. I think their book and the policies toward crime that will develop from it constitute one payoff, an example of the kind of payoff for society the background basic research can provide.

Those are the comments, Congressman Walgren, I would like to start with, and I will be glad subsequently to answer questions.

[The prepared statement of Dr. Coleman follows:]

Testimony Before the Task Force on Science Policy of the House Committee on Science and Technology

September 18, 1985

James S. Coleman
The University of Chicago and NORC

I have been invited to testify about the relation of social science to government and industry on the basis of my experience in this area. Most of my comments will have to do with social policy research related to government, but I will say a few things about the relation to industry as well. I will not comment on social science per se, or what the government can do for the disciplines of social science. I will comment on the reverse question, what social science research does and can do for the government, and for the society.

It is at this point that some persons would stop me to exclaim that social science research does nothing for the society, but does a lot to the society. It brings about bussing, it is responsible for the premature release of criminals, and a host of other mistaken notions. As someone who has been called "the father of bussing," it may be useful for me to respond to this.

Social research directed to policy-relevant questions may well come out with results that favor one side in a social controversy. And in the hands of partisians in the controversy, these results may have influence.

But a comment is relevant here. The results may favor one side, or may favor the other. For example, my recent research results on public and private schools, showing that Catholic schools produce higher achievement in verbal and mathematical skills, were roundly denounced by many of the same persons who had earlier been happy to use the Equality of Educational Opportunity results. As another example, research carried out by sociologists on the negative income tax experiment showed that such a program would sharply increase the divorce rate and reduce the remarriage rate. This evidence, brought out in Senate hearings, was an important, perhaps decisive, influence in killing negative income tax legislation proposed during the Carter administration. Was that a good thing, or bad? Arguments can be made on both sides. But no one disputes the point that it is better to *know* the consequences of such a program for the family than not to know, just as no one would dispute that it is better to know that black children learn more in a middle class school than in a lower class one.

Social science in this respect is similar to natural science. The *knowledge* produced by research is distinct from the *use* to which this knowledge is part. Yet there are few who would say that we should opt for less knowledge.

The examples of social policy research that I have mentioned are illustrations of a first major point: Social policy research has become, over the past twenty years, an important element in social policy. It is unlikely to diminish in quantity or importance. The best evidence for this is a natural experiment that arose in 1981. When the Reagan administration came into office, policies were instituted which resulted in sharp reductions in the budgets for basic or disciplinary research in social science (in NSF, for example). However, there was much less dimunition in the social science contract research called for in RFPs from Federal agencies -- the kind of research I am calling social policy research. The agencies did not find it to their interest to eliminate this research.

The reasons for this are straightforward. First, changes in society have brought the Federal government much more fully into social policy (as distinct from macroeconomic policy or defense policy or foreign policy) than ever before. It has not gotten out of that business, and can hardly do so unless we revert to a much simpler society. Indeed, the continuing changes in society point toward an increase in the need for enlightened social policy. These policies require information, especially information about how well they are working. Second, social science has come to have the capability of providing systematic information relevant to policy. The technology of social research directed to policy-related questions has developed rapidly.

Whether the government in office views social science with suspicion or welcomes it with open arms, its agencies, as well as the Congress itself, need the information it can provide. These needs will grow, as both Federal and state governments find that an increasing share of their total policy concerns are social policy concerns - whether this is youth unemployment, education, efficient organization of health care, transportation policy, housing for low income families, crime control, family policy, or something else.

Thus, the question is not whether there will or will not be social policy research, nor even how much of it there will be, but how it will come to be done. In effect, what we are discussing here is the emergence of a new kind of information-generating activity as an adjunct to government. The kinds of policies made here about how this activity is carried out will have consequences for how other policies, extending far into the future, get made, and for the quality of those policies.

The crucial questions, I believe, are these: 1) How to insure that this activity aids, rather than undercuts, the democratic pluralism of the policy-making process. 2) How to insure that social policy research is done with sufficient independence to be objective, yet sufficient responsiveness to provide relevant information when it is needed. 3) How to insure that the individual skills and organizational capabilities to do this work are available and continue to develop. I will comment briefly on each of these points.

1. Strengthening democratic pluralism: First, it is important to recognize that social policy research is not value free. The questions it poses, the kinds of information it seeks, are predicated upon certain goals and interests. It is of central importance that social policy research obtain information on those questions in an objective and unbiased fashion, but the questions themselves are not value free. The issue of democratic pluralism thus narrows down to two points: One of these is easily addressed: It is insuring that all interested parties -- not just a government agency that contracted for the policy research -- have access to the information, both the original research data and the results of analyses. The Freedom of Information Act of 1974 did much to bring this about, but it is a matter to which further attention should be given. (The General Accounting Office may be the appropriate body for such a responsibility, and it has moved in that direction in recent years.) The second is more difficult to implement, though the principle is clear: insuring that the information obtained is relevant to the interests of all parties affected by a given policy area. It should not be merely what the social scientist carrying out the research believes to be important. It should not be merely what the officials in the government agency

believe to be important. It should be information relevant to the interests of all parties likely to be affected. This is not a trivial task. I should remind you that hearings before Congressional committees is an institution that developed early in the history of the Republic, directed to much the same purpose. That institution however, is predicated on the assumption that all parties have information relevant to their interests, an assumption that is not well met in today's enormously complex social structure.

This principle may be implemented in several ways. One is to require that in RFPs and the resulting contracts for social policy research, there be a pre-research assessment of information needs of interested parties, with the results of the assessment to be used in design of the research itself. Another is to provide for dual analyses, by different sets of researchers, of data which bear on a given policy, and open availability of data to other researchers. There are others; and each to be effective must be carefully designed.

I want to reinterate the importance of having such measures institutionalized, because of their importance for the democratic process. Information becomes an increasingly valuable commodity in modern society, and citizens lose a portion of their rights as citizens if their information rights are not adequately protected.

An implication of this pluralism in design of and access to social policy research is that it will be used not merely by the government body which contracted for it, but by a variety of parties, mostly outside government. In my experience, in fact, social policy research results are more useful to those without authority than to those with the authority to make policy. Nor are they always used in a way that either those in authority nor the researchers would choose, as my examples earlier have suggested. The end result of all this is not a more orderly and "rational" decision-making process by a government agency. The result is, rather, an upgrading of the knowledge base on which policy issues are fought -- and perhaps even an increase in the likelihood of conflict over policy, as more consequences of a policy become known. But the result is also an increase in the quality of the ultimate policy.

Altogether, the information which social policy research produces is a two-edged sword: If the principles of pluralism in design and open access to results are observed, it can strengthen the democratic process and broaden the rights of citizens; if they are not observed, it can restrict those rights.

2. Independence and responsiveness: It is necessary to avoid two extremes in social policy research: One is a research organization that is wholly dependent on a government agency. The best example is the so-called "beltway bandit," an organization ordinarily with contracts

from a single agency and with senior staff as former employees of that agency. Most of these organizations lack the independence necessary to provide information that may be threatening to the agency that feeds them. The other extreme is university professors. Professors in a university setting are too independent; they are unlikely to provide research results on time, and when the results do arrive, they are likely to be on a different topic. Both of these qualities are fine for basic research in a discipline, but not for research relevant to policy.

A delicate balance of independence and responsiveness is necessary both in the research organization and in its staff. It will necessarily be supported largely by contract funds for specific projects, but not wholly so. There should be some longer term and more secure funding. It can appropriately use university professors, but on its terms, not theirs. It should be close to a university, yet with some organizational distance. I regard the organization in which I do research, NORC, as something of a model, but perhaps the best models may be found in the physical sciences, in the laboratories set up during World War II: JPL at Caltech, Argonne Labs at the University of Chicago, Lincoln Labs at MIT, APL at Johns Hopkins, and a few others. The Federal government has established R & D centers in some areas of social policy, for example, education, and this is a policy that should be expanded.

Another institution that might be usefully considered would involve initiation by Congress itself of social policy research, partly in place of, partly in addition to that which is initiated by executive branch agencies. I will not go into the reasons why this might be a good idea, beyond two points: First, since policy is made in Congress through legislation, then Congress is a natural locus for such research; and second, the multi-headed character of Congress which for some purposes is a defect is an asset here, for it can help insure the pluralism of social policy research design.

If this avenue were followed, the best mechanism would be through an expansion of the role of the Congressional Budget Office, which Alice Rivlin admirably brought into being some years ago. In this case, Congress, through CBO, would contract for, and oversee the execution of, social policy research in much the same way that executive branch agencies currently do. Evaluation of social programs initiated at the Federal level by Congressional legislation would be initiated by Congress, and contracted out and overseen by CBO.

There are various pros and cons to a shift of certain social policy research from the executive to the legislative branch; but it might be considered as one part of an institutional mix to produce and make use of social policy research information.

3. Development of organizational capabilities and individual skills: There now exist a few organizations capable of carrying out social policy research that involves large-scale data collection. They can be counted on the fingers of one hand. As I indicated earlier, these are close to universities, making some use of their faculties, but much tighter organizationally, and mission-oriented. Most subsist entirely on short-term contracts, and are unable to offer security to their professional staff. Their staffs include many who have learned wholly on the job. The data analysts are usually sociologists, statisticians, or economists; but in none of these disciplines -- nor, I should add, in the graduate schools of policy studies -- does the appropriate mix of training for these policy research jobs exist. Also inappropriately trained are some of the project officers in government agencies who monitor and interact with the projects contracted for by the agency.

With the mixed assortment of backgrounds in both the research organizations and government agencies, this work cannot help but be some distance from the efficiency frontier. What is necessary in order to move closer to that frontier is some commitment to maintaining organizational capital, probably through providing some institutional support; and some commitment to development of the appropriate human capital. There are no engineering or applied science schools in social science; yet these constitute the appropriate model. In their absence, graduate training fellowship programs, involving both coursework and hands-on training in the social policy research organizations, can be a substitute. Without a vigorous move in that direction, there will be little means to move toward the efficiency frontier. What must be recognized is that various government agencies are buying a product which involves new organizational capital and a new mix of occupational skills. Without some investment in maintaining the organizational capital and in training programs, the work will be of both lower quality and higher cost than it need be.

Social science research in industry:

I will say little about this, because Wally Albers with direct experience can tell you much more. Your first question here might be: Why be concerned about social science research in industry? If research and development policy is aimed at making our industry more efficient, then R and D should be focussed on in the physical sciences and engineering, and the luxury of social science may be dispensed with.

My experience, particularly as a member of the General Motors Science Advisory Committee, suggests that such a comment could hardly be more incorrect. Over the past few years that Committee has been concerned, as has GM generally, with the question of how GM could become competitive with the Japanese auto manufacturers. Over and over again, it turned out that the central sources of noncompetitiveness had nothing to do with technical expertise, but everything to do with the way the organization was structured. Even when the issue appeared at first to be one of a technical advantage, say in engine design, it turned out in the end to be organizational. The technical knowledge existed in the organization; but the structure of incentives in organization prevented the technical knowledge from ending up incorporated into the engine design.

This has begun to be recognized. For example, the adoption of what GM calls "Quality of Work Life" circles in many plants address this problem for production workers, by engaging their direct involvement in their own jobs. Yet unlike product or manufacturing changes, where effectiveness is carefully monitored, the necessity for systematic evaluation of such organizational changes is seldom recognized. General Motors, of course, is not unique. My experience would have been similar in any of a wide range of industrial organizations.

Most business organizations have not learned how appropriately to use social science research to address problems of the organization itself. They learned rapidly how to use social science research for their markets, that is market research. Some are learning that social science research can be valuable in other ways, as Wally Albers can demonstrate in other testimony here.

But the secret that is only slowly coming to be recognized is that research on functioning of the organization itself is ordinarily ineffective unless it is done from the outside. If done from the inside, the very challenge to the authority structure that it constitutes leads it in most cases to be swallowed up and vanish with no effect. Yet as the success of takeover attempts in recent years suggests, deficiencies in management exist in many organizations.

Recently, management consulting firms have been used by large corporations to aid in restructuring themselves -- a demonstration of the usefulness of outsiders. Just how well and how fully these firms use well-grounded knowledge rather than relying on managerial fads is not clear; what is clear is that this pattern, i.e., use of others outside its own structure of authority to aid it in change is the principal vehicle through which social science information can affect organizational functioning.

The implications of this illustration from industry for the Task Force are two. The first implication goes back to my point about the necessity for developing human and organizational capital in applied social research. If American industry were without a continuing supply of well-trained engineers and applied scientists, it would be in a progressively poorer competitive position internationally.

The example I have given suggests that the same is true for well-trained applied social scientists. Yet the encouragement and support for such training is nearly absent, neither provided by the disciplines of social science (which typically prepare people for teaching) nor provided by training grant programs. Some pump-priming is necessary here, to get such training programs initiated and moving. The second implication of the industry example is quite simple: It is becoming increasingly clear that for large business organizations no less than the policy areas of government, applied social research is a necessary adjunct to effective decision making.

Basic Research in the Social Sciences

I have said nothing about government support of basic research in the social sciences for its own sake, because my purpose here is to report on the existing and potential contributions of social science research to society. But the neglect of basic social science research will result in serious long-term costs to society. There are important contributions of basic research in the social sciences to social policy. An excellent example of this can be found in a book just published, titled Crime and Human Nature, by a political scientist, James Q. Wilson, and a psychologist, Richard Herrnstein. Wilson and Herrnstein carried out no research on crime for a government agency, nor any original research of their own. What they have done is to bring to bear the results of a wide range of research done largely for other purposes, on the question, What kinds of persons become criminals? They arrive at conclusions that have powerful policy implications. None of their conclusions are merely common sense; none could have been drawn without the vast background of basic research (as well as some policy research directly on crime or criminals) that has developed over the past thirty years. Their book, and the policies toward crime that will develop from it, constitutes one payoff for society of this background of basic research.

I would be pleased to answer any questions.

Mr. WALGREN. Thank you very much, Dr. Coleman. We appreciate that.

Let's turn to Dr. Newhouse. I think that is the order in which I went through their names. So, Dr. Newhouse and Dr. Albers and then Dr. Bray.

Dr. Newhouse.

STATEMENT OF DR. JOSEPH P. NEWHOUSE, HEAD, ECONOMICS DEPARTMENT, THE RAND CORP., SANTA MONICA, CA

Dr. NEWHOUSE. Thank you, Chairman Walgren and members of the committee.

The task of charting the Federal policy toward social science research, let alone toward scientific research generally, is daunting. I am not going to try to be comprehensive, but I will try to comment on one way in which I think those who support social science research, and particularly the Federal agencies that support it, can obtain value for their money.

I want to argue that the Federal Government probably underinvests in large-scale, long-term projects, both randomized experiments and observational studies, and that probably portions of agency budgets should be set aside for such projects.

My views on this subject have been shaped by my experience in the past 14 years, during which time I have directed a large experiment in the financing and organization of medical care services. This project, which is unusual in the social sciences in terms of its scope, scale, and duration, has involved over 7,700 individuals in 6 different areas of the country. Since 1971, around $80 million of Federal funds have gone into this endeavor.

Because I am touting experiments, I wish first to establish that this experiment was useful. The experiment sought to provide answers to decades-old questions about whether consumers should pay for some portion of any medical care services they consumed. For years this was debated at an ideological level, with the right insisting that patients would overuse services if they were free, and the left equally adamant that money should not be a barrier to seeking needed services. We now know much more about the consequences of alternative levels of patient cost-sharing, including none. I cite some conclusions to indicate the nature of what has been learned.

Not surprisingly, the less families pay for their medical care, the more they use. Specifically, families who receive medical care free of charge use about 40 percent more services than families who pay the first $1,000 of their annual expenditure.

This increase in use is spread throughout all income and education groups.

Third, the increase in services brought a few measurable improvements in health outcomes, but they were modest. Poor individuals with high blood pressure were more likely to have their condition diagnosed and therefore had on average about a 3 millimeter of mercury reduction in diastolic blood pressure. This, in turn, reduced their annual chance of death by about 8 percent. Overall, however, there was no detectable improvement in the health of the typical person, and statistical precision was high enough to rule out all but the most modest of effects.

In terms of the old debate, there is thus some evidence for both sides. Free care did promote the use of some "needed" services for the unfortunate who were both poor and sick. But free care also induced the use of services with minimal or no benefit. Individuals with cuts who went to emergency rooms are a case in point. Persons with more serious cuts, those that required stitches, were equally likely to seek care regardless of how well they were covered. Persons with less serious cuts, those that did not require stitches, were 63 percent more likely to seek care if it was free.

On balance, the data suggest that making all medical services free is not a cost effective way to improve health. More targeted programs, aimed at certain problems such as high blood pressure, seem a more sensible strategy. Specifically, the modest improvements in health outcomes that resulted from free care could have been realized at much lower cost if subsidized care were targeted on these health deficiencies.

Fourth, part of the experiment also addressed the effects of Health Maintenance Organizations, or HMO's. Although many believe that HMO's deliver cheaper care, skeptics charged that HMO's only appeared to cost less because they enrolled healthier individuals who needed fewer services. We randomly assigned individuals to a traditional HMO and to the fee-for-service system. Neither group had to pay for services. We found that the group in the HMO over a 5-year period had 40 percent fewer hospital admissions, which translated into a dollar savings of 28 percent.

Much has been learned from the moneys spent on this experiment. But did it matter? What use has been made of this information? First, in a period immediately following the publication of these results, there was, in fact, a considerable change in both the financing and organization of medical care services, particularly the use of cost sharing markedly increased. For example, the number of large employers whose health insurance plan had a deductible for inpatient hospital expense rose from 30 percent in 1982 to 63 percent in 1984. Over the same 2-year period the number of employers with an annual deductible of $200 per family or more rose from 4 percent to 21 percent. Some of these employers explicitly cited the experimental results in brochures distributed to their employees to explain the changes. During 1984 the proportion of the population enrolled in HMO's, after rising rather slowly for several years, jumped from 5 to 7 percent. Although it is generally impossible to draw a direct link between the experimental results and these changes other than the brochures, there is little doubt in my mind that there is a connection.

Even if there is a connection, were the results worth $80 million? If, in fact, the experiment promoted these changes, a good case can be made. For example, hospital admission rates, after rising for two decades, began to fall in 1982; total admissions to hospitals among the under 65 fell 7.2 percent between 1982 and 1984; a portion of this fall is undoubtedly due to the rise in cost sharing. Although it is impossible to estimate just how much of the fall is due to increase in cost sharing because we don't know exactly how much those increases were or even how many dollars the 7 percent fall saved, about $100 billion was spent on hospital care by those under 65 in 1983. If this total was 7 percent less than it otherwise would have been, the savings in 1 year were almost 100 times as large as the entire cost of the experiment.

What lessons do I draw? An experiment, if well done, has many advantages. Among them are, first, one can be relatively confident about the findings. For example, with the existing data we could not be sure how much, if any, of the observed lower use of services at HMO's was due to a possibly different mix of enrollees. The HMO's had healthier people involved.

Second, one can study certain issues that cannot be studied by analyzing or collecting data. To ascertain what happened to the health of individuals on various insurance plans, one would need to follow people over a period of several years while they remained on the same insurance plans. Many people change insurance plans when they change jobs. It would have been virtually impossible to say anything about how cost sharing affected health status without a project very much like the experiment, yet without such information it would be hard to say whether the decrease in use was a good thing or a bad thing. Indeed, this was precisely what the left and right debated.

Third, experiments may help to defuse some of the fervor and raise the level of the debate surrounding an issue by giving all parties a better understanding of matters they are otherwise inclined to declaim and exclaim about.

Despite these advantages, experiments are quite rare. In my view they are too rare. I believe a certain modest portion, perhaps 10 to 20 percent of research budgets for applied public policy research, should be set aside for randomized experiments.

Why are the experiments so rare? I can speculate on several reasons.

First, they take a long time to yield results. Many in Government have a very short-term perspective. If a study will not yield results in a matter of months, it is not worth very much. As a result, the Government may systematically underinvest in longer term projects. My son's elementary school has a sign over its front door: "Anything worthwhile takes time." That may be a bit of an overstatement, but it certainly contains a grain of truth. Short-term studies may not narrow one's uncertainty very much.

Second, experiments appear expensive. I use the verb "appear" advisedly. First, the expensive part of most experiments is typically collection of data. One alternative, which is relatively inexpensive, is analysis of existing data. That's a viable option and will save money and time, but the data, those existing data will not provide answers. If new data must be collected, the additional costs of a randomized experiment are probably rather low. The cost must be judged relative to benefits. If a $100,000 study does not reduce one's uncertainty, the $100,000 has been wasted.

Third, research budgets are strained and supporting experiments will require concentrating research budgets in those few institutions that are carrying out the experiment. It is politically attractive to most agencies which support research, including both private and public sources, to support numerous researchers in many parts of the country. Pursuit of this strategy leads to numerous small grants or contracts, many of which produce little. Support of some projects on a larger scale is thus politically risky. It is also inherently risky. Because research is uncertain and human institutions are imperfect, supporting a large experiment puts many eggs in one basket. As a result, I suggest only putting a rather modest part of the budget into such projects. The danger, however, is that in the current environment no large projects will be supported.

I want to turn for a minute to observational studies. Although I believe observational studies have important limits for generating results and insights relevant to policy, the bias that leads to undersupport of large-scale experiments also applies to large-scale observational studies. Just as I did in the case of experiments, I would like to just establish the value of large-scale observational studies by means of an example, and I will use the panel study of income dynamics, which has been carried out since 1968 by the Institute for Survey Research at the University of Michigan, with support from NSF, other Federal agencies, and with some private funds.

The unique and costly aspect of this survey is its following of a cohort of individuals through time; much social science data collection is limited to one point in time. The effort that has gone into the collection and analysis of these longitudinal data has yielded a markedly different conception of poverty than was held two decades ago. Then the common perception was that a group in our society, the "poor", was more or less permanently consigned to be disadvantaged. This view was based in part on the annual statistics from the Census Bureau, which showed that the distribution of income changed little from year to year. This seeming lack of change in fact masked much mobility out of and, unfortuntely, into poverty. The panel study data showed that if one uses the official definition of poverty, 24.4 percent of the U.S. population was poor in one or more of the 10 years between 1969 and 1978, but only 2.6 percent were persistently poor; that's poor in 8 or more of the 10 years. This suggests that fears that the welfare system may permanently trap a large underclass are largely unfounded.

Although this project is an example of the value of large-scale research, it was and is being carried out, and hence does not directly support my claim that there is a bias against large-scale projects; indeed, its existence appears to contradict my claim. Nonetheless, support for the continuation of this project is far from assured, in part because its costs bulk large when compared with the social science research moneys available to the NSF.

I conclude my testimony with two examples of possible experiments that might be carried out in the future. Both the examples are for health policy. In the interests of brevity I think I will not describe them. They are in my written testimony. I used health policy principally in my examples because that is the area I know best, but I have no reason to believe that health policy is different from other policy areas. In biomedical research we routinely support large randomized trials of new drugs and new procedures. We might even profitably expand the use of trials even in biomedical research, but the tradition of such trials is well established, and no biomedical researcher doubts their usefulness. There is not a similar tradition in social science research. I believe we would be better off if there were.

[The prepared statement of Dr. Newhouse follows:]

EXPERIMENTATION AS PART OF A SOCIAL SCIENCE RESEARCH STRATEGY

Joseph P. Newhouse

Testimony before the Subcommittee on Science, Research, and
Technology, Committee on Science and Technology,
The United States House of Representatives,
September 18, 1985

Mr. Chairman, members of the Committee, thank you for the privilege of appearing before this distinguished committee today. My name is Joseph P. Newhouse. I am head of the economics department of The Rand Corporation in Santa Monica, California. The views I present, however, are my personal views and do not necessarily represent those of The Rand Corporation nor any sponsors of its research.

The task of charting a federal policy toward social science research--let alone toward scientific research generally--is a daunting one. I will not try to be at all comprehensive, but instead concentrate on one way in which I think those who support social science research, and particularly federal agencies that support it, can obtain value for their monies. I shall argue that the federal government probably underinvests in large-scale long-term projects, both randomized experiments and observational studies, and that probably portions of agency budgets should be set aside for such projects.

THE RAND HEALTH INSURANCE STUDY

My views on this subject have been shaped by my experience of the past fourteen years, during which time I have directed a large experiment in the financing and organization of medical care services, sometimes referred to as The Rand Health Insurance Study. This project--unusual in social science in terms of its scope, scale, and duration--has involved over 7700 individuals in six different areas of the country. Since 1971 around $80 million of federal funds have gone into this endeavor, first from the Office of Economic Opportunity and since 1973 from the Office of the Assistant Secretary for Planning and Evaluation in DHHS.

Because I am touting experiments, I wish first to establish that this experiment was useful. The experiment sought to provide answers to decades-old questions about whether consumers should pay for some portion of any medical care services they consumed. For years this was debated at an ideological level, with the right insisting that patients would overuse services if they were free and the left equally adamant that money should not be a barrier to seeking needed services. We now know much more about the consequences of alternative levels of patient cost sharing, including none. I cite some conclusions to indicate the nature of what has been learned.

1. Not surprisingly, the less families pay for their medical care, the more they use. Specifically, families who receive medical care free of charge use about 40 percent more services than families who pay the first $1000 of their annual expenditure.

2. This increase in use is spread throughout all income and education groups. In the experiment, however, there was a ceiling on out-of-pocket expenditure ($1000), which was reduced for the poor (it was 5, 10, or 15 percent of family income if that was less than $1000); if the cost sharing had not been reduced for the poor, one can infer that their response to variation in cost sharing would have been greater.

3. The increase in services brought a few measurable improvements in health outcomes, but they were modest. Poor individuals with high blood pressure were more likely to have their condition diagnosed and therefore had on average about a 3 millimeter of mercury reduction in diastolic blood pressure. This in turn reduced their annual chance of death by about 8 percent. There was a small improvement in corrected vision; if care was free, about 10 percent of adult individuals had functional far vision of 20/40 or worse (the cutoff in most states for obtaining a driver's license), whereas about 12 percent of individuals who had to pay something for medical care had vision of 20/40 or worse. Decayed teeth were more likely to be filled if care were free (on average there was about 1 less decayed tooth and 1 more filled tooth if care were free), and the health of the gums was somewhat better if care was free. Overall, however, there was no detectable improvement in the

health of the typical person, and statistical precision was high enough to rule out all but the most modest of effects.

In terms of the old debate, there is thus some evidence for both sides. Free care did promote the use of some "needed" services for the unfortunate who were both poor and sick. But free care also induced the use of services with minimal or no benefit. Individuals with cuts (lacerations) who went to emergency rooms are a case in point. Persons with more serious cuts, those that required stitches (sutures), were equally likely to seek care regardless of how well they were covered. Persons with less serious cuts, those that did not require stitches, were 63 percent more likely to seek care if it was free.

On balance, the data suggest that making all medical services free is not a cost effective way to improve health. More targeted programs, aimed at certain problems (e.g., high blood pressure), seem a more sensible strategy. Specifically, the modest improvements in health outcomes that resulted from free care could have been realized at much lower cost if subsidized care were targeted on these health deficiencies.

4. Part of the experiment also addressed the effects of Health Maintenance Organizations. Although many believe that HMOs deliver cheaper care, skeptics charged that HMOs only appeared to cost less because they enrolled healthier individuals who needed fewer services. We randomly assigned individuals to a traditional HMO and to the fee-for-service system. Neither group had to pay for services. We found that the group in the HMO over a 5 year period had 40 percent fewer hospital admissions, which translated into a dollar savings of 28 percent. Thus, we have established that at least one HMO practiced a much less hospital intensive style of medicine than fee-for-service medicine. The health status consequences of this style and the satisfaction of its users are now being analyzed.

Thus, much has been learned from the monies spent on this experiment. But did it matter? What use has been made of this information? First, in the period immediately following the publication of these results, there was a considerable change in both the financing and organization of medical care services. The use of cost sharing markedly increased. For example, the number of large employers whose health insurance plan had a deductible for inpatient hospital expenses rose from 30 percent in 1982 to 63 percent in 1984. Over the same two year period the number of employers with an annual deductible of $200 per family or more rose from 4 percent to 21 percent. Some of these employers (e.g., Xerox, Quaker Oats) explicitly cited the experimental results in brochures distributed to their employees to explain the changes. During 1984 the proportion of the population enrolled in HMOs, after rising rather slowly for several years, jumped from 5 to 7 percent. Although it is generally impossible to draw a direct link between the experimental results and these changes other than the brochures, there is little doubt in my mind that there is a connection.

Even if there is a connection, were the results worth $80 million? If in fact the experiment promoted these changes, a good case can be made. For example, hospital admission rates, after rising for two decades, began to fall in 1982; total admissions to hospitals among the under 65 fell 7.2 percent between 1982 and 1984; a portion of this fall is undoubtedly due to the rise in cost sharing I just noted. Although it is impossible to estimate just how much of the fall is due to increases in cost sharing or even how many dollars the fall saved, about $100 billion was spent on hospital care by those under 65 in 1983. If this total was 7 percent less than it otherwise would have been, the savings in one year were almost 100 times as large as the entire cost of the experiment.

LESSONS DRAWN FROM THIS EXAMPLE

An experiment, if well done, has many advantages. Among them are:

1. One can be relatively confident about the findings. If, for example, we had simply studied use among those with varying degrees of health insurance coverage, we would have observed that those with better coverage would have used more services. But we would not have known how much of the increase was due to sicker individuals obtaining more complete coverage, just as we could previously not be sure how much, if any, of the observed lower use of services at HMOs was due to a possibly different mix of enrollees (i.e., a healthier group).

2. One can study certain issues that simply cannot be studied by collecting or analyzing existing data. For example, to ascertain what happened to the health of individuals on various insurance plans, one would need to follow people over a period of several years while they remained on the same insurance plans. Many people change insurance plans when they change jobs or even at times of open enrollment in plans like the federal employees plan. It would have been virtually impossible to say anything about how cost sharing affected health status without a project very like the experiment, yet without such information it would be hard to appraise whether the decrease in use was a good thing or a bad thing. Indeed, this was precisely what the left and right debated.

3. Experiments may help to defuse some of the fervor and raise the level of the debate surrounding an issue by giving all parties a better understanding of matters they are otherwise inclined to declaim and exclaim about.

Despite these advantages, experiments are quite rare. In my view, they are too rare. I believe a certain modest portion--perhaps 10 to 20 percent of research budgets for applied public policy research--should be set aside for randomized experiments.

Why are experiments so rare? I can speculate on several reasons:

1. Experiments take a long time to yield results. Many in government have a very short term perspective; if a study will not yield results in a matter of months, it is not worth very much. As a result, the government may systematically underinvest in longer-term projects. My son's elementary school has a sign over its front door: "Anything worthwhile takes time." That may be a bit of an overstatement, but it certainly contains a grain of truth; short term studies may not narrow one's uncertainty very much. Another risk of a lengthy experiment is that by the time the results are in, the policy problem may have changed. If one is going to mount an experiment, therefore, it is important to choose problems that are enduring. The health insurance example is a good one. How much patients should pay for medical care is a problem with which virtually every Western democracy (and even the People's Republic of China) continues to wrestle.

2. Experiments appear expensive. I use the verb "appear" advisedly. First, the expensive part of most experiments is collection of data. One alternative, which is relatively inexpensive, is analysis of existing data. If that is a viable option, it will save money and time, but because the data were usually not gathered for the purpose of the study, they will frequently not provide very good answers. If new data must be collected, the additional costs of a randomized experiment are probably rather low. Second, cost must be judged relative to benefits. If a $100,000 study does not reduce one's uncertainty, the $100,000 has been wasted.

3. Research budgets are strained, and supporting experiments will require concentrating research budgets in those few institutions that are carrying out the experiment. It is politically attractive to most agencies which support research (including both private and public sources) to support numerous researchers in many parts of the country. Pursuit of this strategy leads to numerous small grants or contracts, many of which produce little. Support of some projects on a larger scale is thus politically risky. It is also inherently risky; because research is uncertain and human institutions are imperfect, supporting a large experiment puts many eggs in one basket. As a result, I suggest only putting a rather modest part of the budget into such projects. The danger, however, is that in the current environment no large projects will be supported.

LARGE SCALE OBSERVATIONAL STUDIES

Although I believe observational studies have important limits for generating results and insights relevant to policy, the bias that leads to undersupport of large-scale experiments also applies to large-scale observational studies. After promoting experiments, however, I want to also say a few words in support of observational studies. Just as I did in the case of experiments, I would like to establish the value of large-scale observational studies by means of an example, the Panel Study of Income Dynamics, which has been carried out since 1968 by the Institute for Survey Research at the University of Michigan, with support from the National Science Foundation, other federal agencies, and some private monies. This survey has followed 5000 American families, as well as their offspring as they left those families, and has collected information about their income, the payments they received from various government programs, and any changes in the makeup of their households, among other things.

The unique and costly aspect of this survey is its following of a cohort of individuals through time; much social science data collection is limited to one point in time. The effort that has gone into the collection and analysis of these longitudinal data has yielded a markedly different conception of poverty than was held two decades ago. Then the common perception was that a group in our society (the "poor") was more or less permanently consigned to be disadvantaged. This view was based in part on the annual statistics from the Census Bureau, which showed that the distribution of income changed little from year to year. This seeming lack of change in fact masked much mobility out of (and unfortunately into) poverty. The Panel Study data showed that if one uses the official definition of poverty, 24.4% of the US population was poor in one or more of the ten years between 1969 and 1978, but only 2.6% were persistently poor (poor in eight or more of the ten years). This suggests that fears that the welfare system may permanently trap a large underclass are largely unfounded. By far the most important cause of change in economic status is not additional education or training but changes in a family's makeup. Such changes especially affect the economic welfare of women. A woman is most likely to move into poverty as a result of a divorce or separation or widowhood; by the same token, she is most likely to escape poverty because of remarriage.

Another common view is that although the majority of individuals have jobs with high pay, security, and chances for advancement, others are trapped in dead-end jobs. Again the longitudinal data of the Panel Survey suggest this view may be overstated. Of those white males between the ages of 25 and 50 (in 1969) whose hourly earnings placed them in the lowest 20 percent of earnings in 1969, less than half were in the lowest 20 percent of earnings in 1978.

Although this project is an example of the value of large-scale research, it was (and is being) carried out, and hence does not directly support my claim that there is a bias against large-scale projects;

indeed, its existence appears to contradict my claim. Nonetheless, support for the continuation of this project is far from assured, in part because its costs bulk large when compared with the social science research monies available to the National Science Foundation.

TWO EXAMPLES OF POSSIBLE LARGE-SCALE EXPERIMENTS

Although observational studies such as the Panel Study on Income Dynamics can be quite valuable--and are sometimes the only type of data that can be used to study a certain question, I would like to return to my theme that the federal government could profitably carry out more large scale randomized experiments. I do this by citing two examples of possible experiments.

Both my examples come from changes that might be made in the Medicare program. The program is now considering changes in the methods it uses to pay physicians. From 1978 to 1983 expenditures for Part B of Medicare, the great majority of which are for physician services, rose by more than 20 percent per year (13 percent adjusted for inflation). At the present time Part B is the largest single domestic program funded from general revenues. Reimbursements to physicians under Part B have now been effectively frozen, although no informed observer believes it is sensible to keep them frozen indefinitely. At the same time, there is no consensus on what should be done with Part B.

Some advocate extending the Prospective Payment System devised for hospitals to physicians. Thus, just as hospitals are paid a fixed amount for treating a heart attack, so too would physicians be paid a fixed amount. Such a method would run considerable risks for patients. Patients differ in their need for physician services; there are mild heart attacks and severe heart attacks, for example. A pure prospective payment system would pay the same amount for both. Severe heart attacks that do not cause death quickly tend to be expensive to treat. Under a prospective payment system, physicians would have a financial incentive to try to induce patients who survived severe heart attacks to seek care from other physicians. Although an ethical physician may not act on such an incentive, the more some physicians do so, the worse the problem becomes for other physicians. The end result may be that patients who are costly to treat have a difficult time finding someone who is willing to treat them.

Others advocate a revised schedule of fees for physicians. This, however, runs the risk that expenditures in Part B may continue to increase at unacceptable rates. Even if fees, for example, were controlled, physicians may simply deliver more services, thereby raising expenditure.

To me the case is rather strong for an experiment. The alternatives are to implement something rather radical on a national scale with all the attendant risks or to only make modest changes with the hope that modest changes will have large beneficial effects. Clearly neither alternative is attractive. Moreover, the basis for paying physicians is an enduring problem. Medicare, just like every other kind of health insurance public or private, must decide the issue, and current data simply do not shed much light on the problem. For example, there is no empirical basis for assessing the extension of the Prospective Payment System to physicians because physicians have never been paid in such a fashion before.

A second example, also from the Medicare program, concerns post-hospital care. Under the new Prospective Payment System, hospitals receive a fixed amount for a given patient; as a result, there is an incentive for earlier discharge of a patient to skilled nursing facilities or to home health care services. This earlier discharge may raise total expenditure, because the hospital still gets on average what it formerly received (to a first approximation), while the nursing home or home health agency (which is still paid per day or per visit) gets more than before. There is a strong temptation to increase the amount paid the hospital and tell the hospital that it is responsible for all post-hospital care. While this should end the incentive to discharge prematurely, it might create other problems; for example, hospitals would have a financial incentive not to treat patients at higher risk of post-hospital care. Again, the only way to tell if adverse effects will materialize is to conduct an experiment.

I have taken my examples from health policy because that is the area I know best. I have no reason to believe, however, that health policy is different from other policy areas. In biomedical research we routinely support randomized trials of new drugs and new procedures. We could in my view profitably expand the use of trials even in biomedical research, but the tradition of such trials is well established, and no biomedical researcher doubts their usefulness. There is not a similar tradition in social science research; I believe we would be better off if there were.

Mr. **Walgren**. Thank you very much, Dr. Newhouse.

Let's turn then to Dr. Albers.

STATEMENT OF DR. WALTER A. ALBERS, JR., HEAD, SOCIETAL ANALYSIS DEPARTMENT, GENERAL MOTORS RESEARCH LABORATORIES, WARREN, MI

Dr. **Albers**. Thank you, Congressman Walgren.

My name is Walter Albers, and I am the head of the societal analysis department at the General Motors Research Laboratories, located at the General Motors Technical Center in Warren, MI. I certainly welcome this opportunity to testify before the Task Force on Science Policy.

A large fraction of the human resources that make up my department represent the social and behavioral sciences. Although a physicist by education and training, I have had 12 years of experience in the practice and management of social and behavioral science research programs. During this period, my responsibilities have resulted in numerous interactions within and outside of my own company on social and behavioral science issues.

My focus this morning will be to share with you some of my observations and experiences concerning the utilization of the social and behavioral sciences in the industrial research community. I emphasize research. To focus on the research end of the spectrum is somewhat limiting. However, the rationale for this choice is that research is the harbinger of the future, and therefore examining the research component of this subject would provide a context for the future as the results of current research move out into the operational activities of industrial organizations.

I would like to start with some general observations. In my opinion, the climate for social and behavioral science research in industry has changed dramatically over the last two decades. Prior to the 1960's society accepted, indeed, encouraged unabated industrial growth. But during the 1960's and 1970's society became sensitized to environmental, energy and health and safety issues and recognized some of the side effects of unquestioned technological growth and development. The public's concern manifested itself in a series of Federal and State regulations and the growth of powerful public interest groups. Society became a much more prominent stakeholder in the industrial concerns that served it. Contrary to the period before the 1960's, social issues are now a dominant factor in shaping the business environment in which industry must operate. This has contributed to a changing climate for research in the social and behavioral sciences from one of the pre-1960's apathy to a 1980's necessity.

There are two other important factors that have contributed to the shift in importance of social and behavioral science research to industry. The first is there is evidence that the education of social and behavioral scientists is slowly moving from an emphasis on theory to an emphasis on application. Thus, current and future graduates will be more problem-oriented and will find their way better into industrial positions. And, second, the emergence of large capacity, fast computers has enabled the efficient management of large data bases, expanding the quantitative, analytical capacities for our subject sciences. This has opened new doors for the application of social and behavioral sciences in the industrial research context. I cannot overemphasize the importance of quantitative research in the business environment. Quantitative research helps overcome the natural distrust of what has been alleged to be the subjective results of much social and behavioral science research.

There are many other factors that have contributed to the changing climate for industrial R&D in the social and behavioral sciences. Many of these have been chronicled by Rosabeth Moss Kanter in her book, *The Change Masters,* and by other prominent authors. Suffice to say that there is strong evidence for a substantially different, more receptive climate for social and behavioral science R&D in industry.

Let me use my own activity in the General Motors Research Laboratories to illustrate these points. Twelve years ago my department was created with a core group of three people: A chemical engineer, a psychologist and myself, a physicist. Since that time we have grown to an interdisciplinary staff of 30, that breaks down the following way: five psychologists, three sociologists, six economists, one cultural anthropologist, four operations research/systems scientists, two applied mathematicians, one biologist, one physicist, one chemical engineer, and four computer scientists, along with two secretarial staff. Fifteen of these individuals represent the social and behavioral sciences, by my count. All are Ph.D's. except for several computer scientists and the secretaries. The social and behavioral scientists are blended with an equal balance of applied mathematicians and physical scientists in the belief that the research on social issues at the interface between the corporation and society truly requires interdisciplinary approaches.

Our list of R&D activities is diverse and long, and includes measurement of social change and scenario building, survey research, technology assessment, societal risk assessment, behavioral research related to such things as community noise, outdoor visibility, driver behavior and risk taking, demographics, some research on human resource items such as personnel benefits and health care and the marketing sciences, particularly with concerns about customer satisfaction and product quality. Very few, if any, of these were subjects of R&D in industrial research laboratories 15 years ago, including GM Research Laboratories.

There has been a growth in the employment of social scientists elsewhere in GM as well. Some of these carry out R&D activities. For example, our economics staff has added a number of economists and other social scientists over the last 10 years, and many members of this staff perform first-class research in their disciplines. I could go on, but I'll stop here hoping I have convinced you that there has been a substantial growth in behavioral and social scientists doing R&D in General Motors.

My observations over the last several years suggest that similar transitions have occurred in other industries' R&D activities in this period as well. According to the National Academy of Science's *1983 Report on Personnel Needs and Training for Biomedical and Behavioral Research,* there were 864 nonclinical Ph.D. behavioral scientists employed by large companies in 1972. By 1981 this had increased to 1,720, an increase of 100 percent. Another way of looking at this is that the growth of Ph.D. behavioral scientists in the decade of the 1970's has been as large as the total number employed in 1972, a phenomenal growth trend. Assuming that a substantial fraction of behavioral scientists in business are carrying out research and development, this amazing statistic points to a rapid growth of behavioral science R&D in business.

On the social sciences side of the issue, I don't have similar data on Ph.D.s, but the National Science Foundation reports that, for all degree levels in sociology, there were 10,900 employed by industry in 1976. By 1983, according to NSF this number had grown to 23,300, an increase of 114 percent in 7 years. As in the case of Ph.D. behavorial scientists, this growth in business-employed sociologists in the 7-year period exceeded the total level of employment in 1976. Again, I have no way of estimating how many of these are employed in an R&D position, but the number could be substantial.

These numbers look even more remarkable when put in the context of total employment in industry during the 1970's. As a nation we witnessed two severe recessions during the 1970's and 1980's, and the total work force in much of U.S. industry actually decreased during this period. And yet during this same period we have experienced the flow against the tide of behavioral and social scientists entering the industrial work force in substantial numbers.

I would like to close my remarks with a couple examples of applications of the social and behavioral sciences in industrial research which I think portend new directions. First, an example from the behavioral sciences. Back a few years ago, we were pursuing ways to reduce community noise. The automobile industry clearly recognized that traffic noise is a component of the community noise problem, but had no reliable, quantitative information to act on. We set out on a research program to quantify, to the extent possible, the impact of traffic sounds on the U.S. population. Two psychologists, an urban planner, a physicist, and a computer programming expert comprised the research team. The psychologists carried out laboratory and field research which, with the help from the physicist who supplied the physical acoustics expertise, revealed some quantitative relationships between traffic sounds and human annoyance. One notable quantification was that a threshold of annoyance was identified. Employing this quantitative information, the computer programmer worked with the team to develop a simulation model of traffic sound's annoyance potential to the U.S. population. The simulation model was then exercised to identify the most cost-effective approaches to reducing public annoyance to traffic sounds. This work was published in a series of papers in the psychology section of the *Journal of the Acoustical Society of America*. A GM benefit of this effort was the contribution to the social action of regulating sound emissions from trucks and buses, thus giving some relief to society from annoyance due to this component of community noise. The research team makeup exemplifies the interdisciplinary approach to research.

A second example relates to the research we are doing in the area of societal risk assessment. Our risk assessment research team is comprised of a sociologist, an economist, a biologist, an applied mathematician, a psychologist, a chemical engineer and yours truly, a physicist. The representation of social and behavioral sciences on this team is essential in view of the fact that much of the societal risk assessment undertaken involves perceived risks and risk-taking behavior within society itself.

A specific aspect of risk we have researched, among many others, is the risk reduction associated with seat belt usage by occupants of passenger cars. Research proved that increasing seat belt usage was an extremely cost-effective way of reducing risk to the motoring public. This led to the development of a positive reward system for motivating seat belt usage which turned out to be effective, regardless of the perceived risk perspective exhibited by the driving public. Indeed, a number of field experiments revealed that seat belt usage could be increased by over 100 percent in small subpopulations by simply offering prizes for people who were observed to be wearing their belts while driving. Several such programs are now being administered at the community level across the country.

These examples demonstrate two major points that I would like to leave with you regarding the future of behavioral and social science research in industry. The key words are interdisciplinary and quantitative. A big future for behavorial and social scientists in industrial research is in those areas where their disciplines are particular complements to the disciplines of the natural sciences and engineering in gathering the knowledge and understanding about how the changing business environment impacts on the way industrial organizations carry out their business and develop their future business plans.

And a key requirement for social and behavioral science inputs will be quantitative support to reduce the amount of uncertainty and maximize the objectivity of their contributions.

I thank you for the invitation to present my views, and I sincerely hope that your task force is productive and rewarding in its accomplishments.

[The prepared statement of Dr. Albers follows:]

PREPARED STATEMENT OF DR. WALTER A. ALBERS, JR., HEAD, SOCIETAL ANALYSIS DEPARTMENT, GENERAL MOTORS RESEARCH LABORATORIES, UTILIZATION OF THE SOCIAL AND BEHAVIORAL SCIENCES BY INDUSTRY

My name is Walter Albers. I am the Head of the Societal Analysis Department at the General Motors Research Laboratories, located at the GM Technical Center in Warren, Michigan. I welcome this opportunity to testify before the Task Force on Government Science Policy.

A large fraction of the human resources that make up my Department represent the social and behavioral sciences. Although a physicist by education and training, I have had twelve years of experience in the practice and management of social and behavioral science research programs. During this period, my responsibilities have resulted in numerous interactions within and outside of my own company on social and behavioral science issues.

My focus this morning will be to share with you some of my observations and experiences concerning the utilization of the social and behavioral sciences in the industrial research community. To focus on the research end of the spectrum is somewhat limiting. However, the rationale for this choice is that research is the harbinger of the future, and therefore examining the research component of the subject will provide a context for the future as the results of research move out into the operational activities of industrial organizations.

I'd like to start with some general observations. In my opinion, the climate for social and behavioral science research in industry has changed dramatically over the last two decades. Prior to the 60's, society accepted - indeed encouraged - unabated industrial growth. But during the 60's and 70's society became sensitized to environmental, energy, and health and safety issues and recognized some of the side effects of unquestioned technological growth and development. The public's concern manifested itself in a series of federal and state regulations and the growth of powerful public interest groups. Society became a much more prominent stakeholder in the industrial concerns that served it. Contrary to the period before the 60's, social issues are now a dominant factor in shaping the business environment in which industry must operate. This has contributed to a changing climate for research in the social and behavioral sciences from one of the pre-60's apathy to an 80's necessity.

There are two other important factors that have contributed to the shift in importance of social and behavioral science research to industry. The first is there is evidence that the education of social and behavioral scientists is slowly moving from an emphasis on theory to an emphasis on application. Thus, current and future graduates will be more problem-oriented and will find their way better into industrial positions. And secondly, the emergence of large capacity, fast computers has enabled the efficient management of large data bases, expanding the quantitative, analytical capacities for our subject sciences. This has opened new doors for the application of social and behavioral sciences in the industrial research context. I cannot over-emphasize the importance of quantitative research in the business environment. Quantitative research helps overcome the natural distrust of what has been alleged to be the "subjective" results of much social and behavioral science research.

There are many other factors that have contributed to the changing climate for industrial R & D in the social and behavioral sciences. Many of these have been chronicled by my distinguished colleague on this panel, Rosabeth Moss Kanter in her book "The Change Masters." Suffice to say that there is strong evidence for a substantially different, more receptive climate for social and behavioral science R & D in industry.

Let me use my own activity at the General Motors Research Laboratories to illustrate these points. Twelve years ago my Department was created with a core group of three people, a chemical engineer, a psychologist and myself, a physicist. Since that time we have grown to an interdisciplinary staff of thirty, that breaks down the following way: five psychologists, three sociologists, six economists, one cultural anthropologist, four operations research/system scientists, two applied mathematicians, one biologist, one physicist, one chemical engineer and four computer scientists, along with two secretarial staff. Fifteen of these individuals represent the social and behavioral sciences. All are PhD's except for several computer scientists and the secretaries. The social and behavioral scientists are blended with an equal balance of applied mathematicians and physical scientists, in the belief that the research on social issues at the interface between the corporation and society truly requires interdisciplinary approaches.

Our list of R & D activities is diverse and long, and includes measurement of social change and scenario building, survey research, technology assessment, societal risk assessment, behavioral research related to such things as community noise, outdoor visibility, driver behavior and risk taking, demographics, some research on human resource items such as personnel benefits and health care and marketing science, particularly with concerns about customer satisfaction and product quality. Very few, if any, of these were subjects of R & D in industrial research laboratories fifteen years ago, including GM Research Laboratories.

There has been growth on the employment of social scientists elsewhere in GM as well. Some of these carry out R & D activities. For example, our Economics Staff has added a number of economists and other social scientists over the last ten years, and many members of this staff perform first-class research in their disciplines. I could go on, but I'll stop here, hoping that I've convinced you that there has been a substantial growth in behavioral and social scientists doing R & D in GM.

My observations over the last several years suggest that similar transitions have occurred in other industries' R & D activities in this period as well. According to the national Academy of Science's 1983 Report on Personnel Needs and Training for Biomedical and Behavioral Research, there were 864 non-clinical PhD behavioral scientists employed by large companies in 1972. By 1981 this had increased to 1720, an increase of 100 percent. Another way of looking at this is that the growth of PhD behavioral scientists in the decade of the seventies has been as large as the total number employed in 1972. Assuming that a substantial fraction of behavioral scientists in business are carrying out research and development, this amazing statistic points to a rapid growth of behavioral science R & D in business.

On the social sciences side of the issue, I don't have similar data on PhD's, but the National Science Foundation reports that, for all degree levels in sociology, there were 10,900 employed by industry in 1976. By 1983, according to NSF, this number had grown to 23,300, an increase of 114 percent in seven years. As in the case of PhD behavioral scientists, this growth in business-employed sociologists in the seven-year period exceed the total level of employment in 1976. Again, I have no way of estimating how many of these are employed in an R & D position, but the number could be substantial.

These numbers look even more remarkable when put in the context of total employment in industry during the seventies. As a nation, we witnessed two severe recessions during the 70's and early 80's, and the total workforce in much of U.S. industry actually decreased during this period. And yet, during this same period, we have experienced the "flow-against-the-tide" of behavioral and social scientists entering the industrial workforce in substantial numbers.

I would like to close my remarks with a couple examples of applications of the social and behavioral sciences in industrial research which I think portend new directions. First, the behavioral sciences. Back a few years ago, we as a society were actively pursuing ways to reduce community noise. The automobile industry clearly recognized that traffic sounds were a component of the community noise problem, but had no reliable, quantitative information to act on. We set out on a research program to quantify, to the extent possible, the impact of traffic sounds on the U.S. population. Two psychologists, an urban planner, a physicist and a computer programming expert comprised the research team. The psychologists carried out laboratory and field research which, with help from the physicist who provided the physical acoustics expertise, revealed some quantitative relationships between traffic sounds and human annoyance. One notable quantification was that a threshold of annoyance was identified. Employing this quantitative information, the computer programmer worked with the team to develop a simulation model of traffic sound's "annoyance potential" to the U.S. population. The simulation model was then exercised to identify the most cost-effective approaches to reducing public annoyance to traffic sounds. This work was published in a series of papers in the psychology section in the Journal of the Acoustical Society of America. A GM benefit of this effort was the contribution to the social action of regulating sound emissions from trucks and buses, thus giving some relief to society from annoyance due to this component of community noise. The research team makeup exemplifies the interdisciplinary approach to research.

A second example relates to the research we are doing in the area of societal risk assessment. Our risk assessment research team is comprised of a sociologist, an economist, a biologist, an applied mathematician, a psychologist, a chemical engineer and, yours truly, a physicist. The representation of the social and behavioral sciences on this team is essential in view of the fact that much of the societal risk assessment undertaken involves perceived risks and risk-taking behavior within society itself.

A specific aspect of risk we have researched, among many others, is the risk reduction associated with seat-belt usage by occupants of passenger cars. Research proved that increasing seat belt usage was an extremely cost-effective way of reducing risk to the motoring public. This led to the development of a positive reward system for motivating seat-belt usage which turned out to be effective, regardless of the perceived risk perspective exhibited by the driving public. Indeed, a number of field experiments revealed that seat-belt usage could be increased by over 100 percent in small sub-populations by simply offering prizes for people who were observed to be wearing their belts while driving. Several such programs are now being administered at the community level across the country.

These examples demonstrate two major points that I would like to leave with you regarding the future of behavioral and social science research in industry. The key words are interdisciplinary and quantitative. A big future for behavioral and social scientists in industrial research is in those areas where their disciplines are particular complements to the disciplines of the natural sciences and engineering in gathering the knowledge and understanding about how the changing business environment impacts on the way industrial organizations carry out their business and develop their business plans for the future. And a key requirement for social and behavioral science inputs will be quantitative support to reduce the amount of uncertainty and maximize the objectivity of their contributions.

I thank you for the invitation to present my views, and sincerely hope that your Task Force is productive and rewarding in its accomplishments.

Mr. **Walgren**. Thank you very much, Dr. Albers.

Then let's turn to Dr. Bray.

[A biographical sketch of Dr. Bray follows:]

Douglas W. Bray, Ph.D.

AT&T CAREER

1976-83: Director—Basic Human Resources Research.

1956-75: Director—Management Selection and Development Research; Director—Personnel Relations and Research; and Director—Manpower Action Program.

EARLY CAREER

1950-55: Research Associate, Conservation of Human Resources, Columbia University.

1984-50: Research Associate, Study of Education, Princeton University.

EDUCATION

American International College, B.A. in Sociology, 1940.
Clark University, M.A. in Psychology, 1981.
Yale University, Ph.D. in Psychology, 1984.

PROFESSIONAL RECOGNITION

Distinguished Contribution to Applied Psychology as a Professional Practice Award, American Psychological Association, 1980.

Professional Practice Award, Division of Industrial/Organizational Psychology, American Psychological Association, 1977.

Distinguished Scholar Award, Hofstra University, 1975.

Achievement Award, Alumni Association, American International College, 1981.

Diplomate in Industrial/Organization Psychology, American Board of Professional Psychology, 1960.

AFFILIATIONS

American Psychological Association—Fellow, Division 8 (Personality and Social), 14 (Industrial/Organizational), 20 (Adult Development and Aging), and 31 (State Psychological Association Affairs).

President—Board of Directors, American Board of Professional Pyschology 1982-83.

President—Division of Industrial and Organizational Psychology of the American Psychological Association, 1971-72.

Chairperson—American Psychological Association Task Force on the Practice of Psychology in Industry, 1967-71.

Member—Committee on Scientific and Professional Ethics and Conduct, American Psychological Association, 1970-78; 1981-83.

Member—Membership Committee, American Psychological Association, 1978-80.

Member—American Psychological Association Task Force on Employment Testing of Minority Groups, 1967-69.

Member—American Psychological Association Council of Representatives, 1967-70.

Member—Planning Committee and Follow-up Commission, Vail Conference on Levels and Patterns of Training in Professional Psychology, 1970-75.

Member—Board of Trustees, Association for the Advancement of Psychology, 1974-75.

Member—Council of Representatives, New York State Psychological Association, 1967-71.

President—Division of Personnel Psychology, New York State Psychological Association, 1969-70.

Member—Department of Defense Manpower Research Task Force, 1970-71.

President—New York Chapter, Duke Ellington Society, 1959-63.

President—East Meadow, New York Public Library Board, 1955-56.

President—East Meadow, New York Citizens' Committee for Public Schools, 1957-58.

Treasurer—Christ the King Episcopal Church (East Meadow, NY), 1965-75.

STATEMENT OF DR. DOUGLAS W. BRAY, CHAIRMAN, DEVELOPMENT DIMENSIONS INTERNATIONAL, TENAFLY, NJ

Dr. **Bray**. Thank you very much for inviting me to testify.

I would like to amplify a little bit my bio-data. It's true that currently I am chairman of Development Dimensions International, but I should point out to the committee, since it is not prominent in the testimony, that I was the director of basic human resources research for AT&T for approximately 28 years until 1983. And most of the examples I am going to give come from that background.

Also, let me say that I plan to read part of this testimony, but I intend to paraphrase and perhaps slightly amplify others; that's for those who are following along as I see nearly everyone in the room is.

I might say further that I am a psychologist and that I am going to talk about psychology. I am also going to try to focus on the relationship between basic research done outside of industry and its application in industry because it seems to me that that was maybe really what the committee was looking for in terms of their focus for this morning.

One further focus is I am going to focus on managers and those—by the term manager, I mean everybody from the first line information person up to the chairman of the board as a manager. And when I say organization, I mean to not merely mean business organizations, although my examples may come more from that, but I plan to—I think that the remarks I am going to make would be very applicable to governmental, military, educational, and other large organizations.

I am going to present two examples—and in this case I may read. I am going to present two examples of the application of basic research in industry. They are not the only examples that could be given; however, they are the two with which I am most deeply acquainted and can speak with the most expertise. One of them is the application of the assessment center method to the selection and development of managers and the other is the application of behavior modeling training to business managers or organizational managers.

Let me now read, starting bottom of page 2.

The assessment center method had its origin in the work of Dr. Henry Murray at the Harvard Psychological Clinic in the early 1930's. Here Dr. Murray suggested that, for the first time, several psychological and psychiatric researchers examine the same group of participants and, meeting together, reach an overall understanding of the personality of those evaluated. This experiment was reported in Murray's classic volume, *Explorations in Personality.* Let me say and emphasize now that Murray's intent at that time was simply basic research. He was a personality psychologist, among many other things, and he was interested in developing basic knowledge of human personality. There was no applied implication at all in Murray's works. He didn't even correlate the results, for example, of the performance of these individuals in assessment and their grades at Harvard. So he really was highly basic and not at all applied.

However, shortly thereafter Murray was invited to come to Washington during World War II to apply this method to the selection of intelligence agents by the Office of Strategic Services, and this very interesting application was reported in the *Assessment of Men* published in 1948. As to the word "Men" in the title, there were actually a few women assessed at that time, but the book was called the *Assessment of Men.*

The assessment center method as used by Murray and his associates involves putting participants through a variety of exercises, both individual and group. These included interviews and tests, but the hallmark of the method was its use of behavioral simulations which confront participants with problems parallel to those which would have to be solved in the job or assignment for which the individual was being evaulated. One of these exercises happens to be an In-Basket, which Murray didn't use, and in my testimony I talk a bit about that. It's a paper simulation of administrative work, and I mention it because that also developed in the military context a fair amount later, but Murray's developed in OSS and the In-Basket was developed in the Air Force. In both cases you can see that there is a transitional situation here between the basic researcher and these things eventually getting applied rather extensively in industry.

The assessment center method was adopted by industry. I was the one who adopted it when I was employed at AT&T in 1956 to start a basic research study in the careers and lives and development of managers. I took it in, following Murray, not completely, but to a large extent, and applied it in a research vein. It was then picked up by one of the telephone companies, Michigan Bell, a couple years later and started its actually being used, not just for research but for actually picking people for first, in that case, first level management jobs. Since then the assessment center has proliferated very widely throughout the United States and, in fact, throughout the world, and it's estimated—no exact count is available, but it's estimated about 2,000 organizations, including governmental organizations, many businesses, the military and so forth have adopted this method and literally hundreds of thousands of managers have undergone the process. In AT&T alone—the count before, unfortunately, AT&T was broken up—the count before that time was 300,000 Bell employees had gone through assessment centers and far more than that outside of Bell had gone through them. It's been used for the selection of managers at all levels, for evaluating the development needs of individuals and for diagnosing organizational training methods, for the evaluation of competence in various professions and even as a tool in changing organizational culture.

The point—I am not going into the next paragraph. There is plenty of quantitative evidence that the assessment center is effective. I am not going to naturally try to present that data today.

The point of that running through the assessment center method is to testify that work that started in a very basic research environment, in fact, does wind up in many cases being widely used in organizations, industrial, governmental and so forth. Let me give one more example—page 5.

Here we start with the work of Dr. Bandura at Stanford and other learning theorists who were interested in finding out how individuals learned good things that they really should be learning, how they could unlearn bad things and, in short, how people developed and came to have the kinds of behavior and responses that they do, in fact, possess. Again, it was a very applied academically-based basic research. In this case—page 5 again—its transition in industry was effected by Dr. Arnold Goldstein of the University of Syracuse and Dr. Melvin Sorcher of General Electric—that was the organization that sponsored the initial bridge between this basic research and applied research in industry. As designed by Goldstein and Sorcher, a behavior modeling module consists of a short set of learning points, or critical steps, to be taken in approaching a supervisory problem with a subordinate; a demonstration of behavior using the learning points presented by film or videotape; and role-playing by the trainee, with another trainee serving as the subordinate to practice carrying out the critical steps. Between training sessions, trainees are often asked to practice the behavior in their own job setting and report back on any difficulties experienced and the results obtained. A full training course would include a number of modules focused on typical supervisory problems encountered at particular levels of management in particular locations.

Once again, this application started by one company started to spread into other places. AT&T was one of them, and by this time in history, about 3,000 organizations now use behavior modeling, and the version marketed by my company, Development Dimensions International, by actual count has been used to train over 1 million people, and we know that there are about 40 firms offering this type of training. They don't all, fortunately, sell a million, but it is a very, very widespread method indeed, and it has been extended to other areas of supervision, particularly to train employees whose jobs entail customer contacts and the specifics to their job situations.

Once again, there is adequate evidence that people do change their behavior. They learn the new behavior as a result of this. They do carry it back and carry it out on the job, and the result is greater productivity, greater attention to the goals of the organization, and so forth.

The next part of my testimony talks about the difficulties in which American industry finds itself—I won't try to prove or adapt to any people on this distinguished committee. I am sure you have your own opinions as to whether American industry is in any kind of trouble or not. I happen to think it is in some trouble, and the attention given to productivity, quality of American goods, international competition, takeovers, divestitures, all kinds of things make, it seems to me, the industrial scene a great deal more turbulent than it certainly used to be. One of my previous mentors, Eli Ginsberg at Columbia University, has just written a book entitled *The Large Corporation at Risk,* in which he, from a somewhat different point of view, also feels that American industry is really, really facing, if not a real crisis, at least that there is plenty of room for improvement.

OK. Then I go on from that to say—bottom of page 7—current thinking sees organizations as small societies or cultures, and there has been much talk of late about changing organizational cultures to encourage the work force to make greater contributions. In addition to these purposeful efforts, the culture of some organizations is being changed by takeovers, divestitures, and other intrusive events. Presumably, changes in the organizational culture are affecting employees for better or worse.

Theorizers about corporate culture often view employees of organizations as all pretty much the same with the same motivations which will cause everyone to respond in the same way to aspects of the culture such as change in the incentive plan or management pronouncements and so forth. This is a gross oversimplification. We know that motivations and attitudes change with age and experience, and that goals and values change from generation to generation. The characteristics of those making up the organization must be taken into account. What we really confront is a complex interaction between the culture of the organization and the people who inhabit it.

OK—how do we get that? What is needed to maximize the effectiveness of America's organizations is basic scientific research leading to methods for organizational change and for the development of those who make up organizations. Some of the areas of this basic knowledge are the status of, and trends in, the abilities and motivations of various segments of the work force, particularly the managerial work force; methods of analyzing organizational cultures and the influence of such cultures on the performance of individuals; and experiments in cultural change. Such basic knowledge would also yield valuable byproducts in the more traditional areas of managerial selection, development, and training.

OK, page 9. I happen to think that basic research of this complexity and magnitude will not be undertaken by businesses on their own. Behavioral scientists employed by industry usually apply existing knowledge rather than develop new scientific principles. Much of what they do may be characterized as factfinding using scientific methods. Administering an attitude questionnaire, for example, may provide useful information to management, but its usefulness is usually purely local. Many firms do not develop the application of existing knowledge, even develop the application of existing knowledge themselves. It is estimated that half of even large organizations purchase needed programs from consulting firms. There is nothing wrong with this, since knowledge is being applied; but I cite it as evidence that industry is a long way from devoting much effort to basic research. One notable exception—and certainly there may well be others; we may have just heard one of them. But one notable exception has been AT&T, which has supported social and behavior science research for the last 30 years, over and above the human engineering work done at the Bell Telephone Laboratories.

Because of the feeling that organizations will not in general and even in any large part support this needed basic research, I believe that it will have to be financed by the Government, at least in large part, and usually directed—that is, the basic research—by those outside of business organizations. Organizations will have to be involved in the research. Business cultures can't be replicated by use of college students as subjects. Although some businesses might readily participate in research at their own expense that they view as worthwhile, others may require financial support for employee time spent or least a tax credit.

The kind of research here envisioned will not be accomplished by 1-year grants to academics. There is much evidence that this is not a good approach. Ambitious measurement methods, such as assessment centers, rather than simple questionnaires will have to be used and, as has often been pointed out, we will never understand human behavior if we rely exclusively on questionnaires and opinion polls. In addition, research will have to be longitudinal, as organizations and their members are followed through time to collect baseline knowledge as well as to study the effects of interventions undertaken.

Although changes might well be made in funding arrangements and standards for research grants so that university-based researchers can play a productive role, it may be that additional avenues should be opened. The possibility of establishing one or more research institutes in this area might be considered.

In any case, scientific research in the social and behavioral sciences needs to be expanded to help preserve and enhance the vitality of organizations, business and otherwise. Organizations are willing and able to apply the results of such research, but they are unlikely to produce it. And for that reason I believe that governmental action and support is needed.

Thank you very much.

[The prepared statement of Dr. Bray follows:]

TESTIMONY OF DOUGLAS W. BRAY, PH.D., THE ROLE OF THE BEHAVIORAL AND SOCIAL SCIENCES

The Task Force on Science Policy has indicated its interest in examining the role of the social and behavioral sciences in some detail because they "occupy a special place in the braod specturm of government science policy". This testimony addresses one aspect of the social and behavioral sciences, the utilization of social and behavior science in the large organizations -- industrial, governmental, and educational -- through which society works to achieve its goals. The testimony does not attempt to reflect all social and behavioral science, but is limited to psychology and, within psychology, to the specialty of Industrial/Organizational psychology.

The testimony wil focus on the managers of organizations. The term "managers" is meant to include the whole spectrum of the organization, from first-line supervisors up to the chief executive officer. This focus is chosen because it is the area of the witness's main experience and because it affords excellent examples of the utilization of behavioral science knowledge and methodology in the "real world". Although a focus on managers will ignore some important applications of behavioral science in organizations, such as in technical skills training and equipment design, the area of management is of great importance. The quality of management in the nation's organizations is of obvious signiicance to all aspects of national well-being.

One key area in regard to managers is that of management staffing, including the initial selection of persons to be managers, the appraisal of their performance, the evaluation of their potential, and selection for advancement in the organization. Another is the area of training and development of managers, including training in management skills, career pathing, and the provision of job challenge and other developmental experiences. Then there is the area of managerial performance itself and its relationship to the productivity of the organization and the well-being of subordinates. Unederlying all these are questions concerning the characteristics of managers themselves, their abilities, motivation, values, and attitudes.

The contributions of behavioral science to these several areas in the management of organizations have been appreciable. No attempt will be made here to catalog these contributions. An excellent reference in this regard is the Handbook of Industrial and Organizational Psychology (Dunnette, 1976).

Instead of attempting such diffuse coverage, two examples of the use of behavioral science knowledge and methods will be explored. Both had their origins in academic research settings, both were then developed in organizational contexts, and both were applied widely in a great many organizations and are still flourishing today. They are the assessment center method and behavior modeling training.

The assessment center method had its origin in the work of Dr.

Henry Murray at the Harvard Psychological Clinic in the early 1930s. Here Dr. Murray suggested that, for the first time, several psychological and psychiatric researchers examine the same group of participants and, meeting together, reach an overall understanding of the personality of those evaluated. This experiment and a theory of personality were reported in Murray's classic volume, Explorations in Personality (1938). Somewhat thereafter Murray was invited to come to Washington during World War II to apply his method to the problem of selecting intelligence operatives by the Office of Strategic Services (OSS). This first application of the method was fully reported in Assessment of Men (OSS Assessment Staff, 1948).

The assessment center method as used by Murray and his associates involves putting participants through a variety of exercises, both individual and group. These exercises include interviews and tests, but the hallmark of the method is its use of behavioral simulations which confront participants with problems parallel to those which would have to be solved in the job or assignment for which the individual is being evaluated. For example, the assessee may be given an In-Basket made up of letters, memos, records of telephone calls, reports, appraisal forms, etc., representative of managerial work at a certain level. He or she must deal with these matters as they believe they would in real life and are then interviewed about their perceptions, decisions, plans, etc. The written material plus the interview enable the assessors to evaluate such skills as delegation, planning and organizing, and decision making. The In-Basket was not part of the Murray methodology but was also developed, as was the assessment center, in a governmental context, the U.S. Air Force (Fredericksen, Saunders, & Wand, 1957).

The adoption of the assessment center method by industry began with its use in research on managerial careers at AT&T, initiatied by myself in 1956 (Bray, Campbell, & Grant, 1974). In 1958 one of the AT&T companies, Michigan Bell, adapted the research center to aid in the selection of non-management employees for first-level supervisory jobs. The method then spread to other Bell System companies and, in 1961, began to diffuse to other organizations.

At present, the assessment center method is used by approximately 2,000 organizations, and hundreds of thousands of managers and others have undergone the process. It has been used for the selection of managers at all levels, including those in the military, education, and the government; for evaluating development needs of individuals and diagnosing organizational training needs; for the evaluation of competence in various professions; and even as a tool in changing organizational culture.

An impressive body of research had demonstrated that the assessment center has significantly improved the quality of management when used as an aid in selection or advancement decisions. Trainers have concluded that their development efforts are more effective when preceded by the more comprehensive picture of strengths and weaknesses provided by assessment. The use of the assessment center in research has delineated the relationship between success in management and a pattern of abilities and motivations, how these abilities and motivations change with experience in management, and how social change has affected these characteristics in the younger generation of managers (Howard & Wilson, 1982).

Behavior modeling training took its inspiration from the work of Dr. Albert Bandura of Stanford University (1969) and other learning theorists. Its transition into industry was effected by Dr. Arnold Goldstein of the University of Syracuse and Dr. Melvin Sorcher, then of General Electric, the organization which supported this initial endeavor. As designed by Goldstein and Sorcher (1974), a behavior modeling module consists of a short set of learning points (or critical steps) to be taken in approaching a supervisory problem with a subordinate; a demonstration of behavior using the learning points presented by film or videotape; and role playing by the trainee (with another trainee serving as the subordinate) to practice carrying out the critical steps. Between training sessions trainees are often asked to practice the behavior in their own job setting and report back on any difficulties experienced and the results obtained. A full training course would include a number of modules focused on typical supervisory problems encountered at particular levels of management in particular locations.

This behavior modeling application spread first to other large organizations, such as AT&T, and then much more generally, following the same sort of pattern as the assessment center but at an accelerated rate. It is estimated that over 3,000 organizations now use behavior modeling. The version marketed by my firm, Development Dimensions International, has been used to train over one million people, and we are only one of the firms providing such programs. Behavior modeling has been extended to other than the area of supervision, particularly to train those employees whose jobs entail customer contacts in interpersonal skills specific to their job situations.

Although the research data are not as extensive as in the case of assessment centers, there is adequate hard evidence that supervisors trained by behavior modeling learn beahviors that are much more effective in carrying out their jobs and that these behaviors are actually used on the job, not just mastered in the classroom. Such training has been readily accepted by organizations since it works directly on behavior and not on imparting theory, which one can only hope will somehow influence behavior.

Large organizations have long been beset by problems such as a shortage of capable managers, errors in promotion decisions, and managerial styles which fail to enlist the full effort of employees. There are indications, however, that additional difficulties are afflicting American industry. It is said to be less productive than those of some other nations, notably Japan, and American goods are often judged to be of less than top quality. American workers, at least many of them, are willing to believe that Japanese workers are more work involved than they are and that they try harder to do well.

At the same time American industry is experiencing a wave of takeovers, mergers, divestitures, and down-sizings which render jobs, even middle-management jobs, far less secure than they once were. Identification with corporations that are unstable in their ownership or even their line of business is difficult and fragile. Then there is the changing nature of the work force, even among those embarking on managerial careers. The motivations to advance and to demonstrate leadership are not as strong as they used to be, and alternative motivations are not readily elicited by hierarchical organizations.

All of this indicates that American businesses are in more difficulty than they often have been and suggest that some of the human problems involved bode ill for the future. Many of these problems are those which require advances in social and behavioral science for their solution. Although various interventions are now being attempted, they are not the products of basic research and their usefulness is problematical.

Current thinking sees organizations as small societies or cultures, and there has been much talk of late about changing organizational cultures to encourage the work force to make greater contributions. In addition to these purposeful efforts, the culture of some organizations is being changed by take-overs, divestitures, and other intrusive events. Presumably, changes in the organizational culture are affecting employees for better or worse.

Theorizers about corporate culture often view employees of organizations as all pretty much the same with the same motivations which will cause everyone to respond in the same way to aspects of the culture. Yet it is clear that this is a gross oversimplification. We know that motivations and atitudes change with age and experience, and that goals and values change from generation to generation. The characteristics of those making up the organization must be taken into account. What we really confront is a complex interaction between the culture of the organization and the people who inhabit it.

What is needed to maximize the effectiveness of America's organizations is basic scientific knowledge leading to methods for organizational change and for the development of those who make up organizations. Some of the areas of this basic knowledge are the status of and trends in the abilities and motivations of various segments of the work force, particularly the managerial work force; methods of analyzing organizational cultures and the influence of such cultures on the performance of individuals; and experiments in cultural change. Such basic knowledge would also yield valuable by-products in the more traditional areas of managerial selection, training, and development.

Basic research of this complexity and magnitude will not be undertaken by businesses on their own. Behavioral scientists employed by industry usually apply existing knowledge rather than developing new scientific principles. Much of what they do might be characterized as fact-finding using scientific methods. Administering an attitude questionnaire, for example, may provide useful information to management, but its usefulness is usually purely local. Many firms do not even develop the application of existing knowledge themselves. It is estimated that half of even large organizations purchase needed programs from consulting firms. There is nothing wrong with this, since knowledge is being applied, but it is evidence that industry is a long way from devoting much effort to basic research. One notable exception has been AT&T, which has supported social and behavioral science research for the last 30 years, over and above the human engineering work at the Bell Telephone Laboratories (Bray & Howard, 1983).

Basic research of the type being discussed here will have to be financed by the government, at least in large part, and usually directed by those outside of business organizations. Organizations must, however, be strongly involved in the research. Business cultures can't be replicated by using college students as subjects. Although some businesses might participate readily in research they view as worthwhile, others may require financial suport for employee time spent or at least a tax credit.

The kind of research here envisioned will not be accomplished by one-year grants to academics. There is much evidence that this is not a good approach (Wachtel, 1980). Ambitious measurement methods, such as assessment centers rather than simple questionnaires, will have to be used. As has often been pointed out (McClelland, 1985), we will never understand human behavior if we rely exclusively on questionnaires and opinion polling. In addition, research will have to be longitudinal, as organizations and their members are followed through time to collect baseline knowledge as well as to study the effects of interventions undertaken.

Although changes might well be made in funding arrangements and standards for research grants so that university-based researchers can play a productive role, it may be that additional avenues should be opened. The possibility of establishing one or more research institutes might be considered.

In any case, scientific research in the social and behavioral sciences needs to be expanded to help preserve and enhance the vitality of organizations, business and otherwise. Organizations are willing and able to apply the results of such research, but they are unlikely to produce it. Governmental action is required.

References

Bray, D.W., Campbell, R.J., & Grant, D.L. (1974). Formative years in business: A long-term AT&T study of mangerial lives. New York: John Wiley & Sons.

Bray, D.W. & Howard, A. (1983). THe AT&T longitudinal studies of managers. In K.W. Schaie (Ed.), Longitudinal studies of adult psychological development (pp. 266-312). New York: Guilford Press.

Dunnette, M.D. (Ed.) (1976). Handbook of industrial and organizational psychology. Chicago: Rand McNally.

Frederiksen, N.O., Saunders, D.R., & Wand, B.I.M. (1957). The In-Basket test. Psychological Monographs, General and Applied, 71 (9, Whole No. 438).

Goldstein, A.P. & Sorcher, M. (1974). Changing supervisor behavior. New York: Pergamon Press.

Howard, A. & Wilson, J.A. (1982). Leadership in a declining work ethic. California Management Review, 24, 33-46.

McClelland, D.C. (1985). How motives, skills, and values determine what people do. American Psychologist, 40, 812-825.

Murray, H.A. (1938). Explorations in Personality. New York: Oxford University Press.

Office of Strategic Services (1948). Assessment of Men. New York: Rhinehart.

Thornton, G.C. III & Byham, W.C. (1983) Assessment centers and managerial performance. New York: Academic Press.

Wachtel, P.L. (1974). Investigation and its discontents: Some constraints on progress in psychological research. American Psychologist, 35, 399-408.

Mr. **Walgren**. Thank you, Dr. Bray.

Then we will turn now to Dr. Abt.

[A biographical sketch of Dr. Abt follows:]

Dr. Clark C. Abt

Clark Abt is founder, President, and Chairman of Abt Associates Inc., one of the largest private social and economic policy research firms in the United States. He has over thirty years' experience directing Government and industry engineering, evaluation, research, publishing and management training. He has directed industry and Government research programs in defense, economic development, education, employment and job training, energy, environment, health and child care, housing, justice, and transportation.

Clark Abt was formerly at the Raytheon Company as Manager of the Advanced Systems and Strategic Studies Departments, and senior operations analyst. He served as a navigator, ECM and intelligence officer with the United States Air Force for five years. Before that, he was employed as an engineer at Bechtel Corporation and an instructor at Johns Hopkins University.

He graduated from M.I.T. in engineering in 1951, received an M.A. in philosophy from Johns Hopkins University in 1952, and a Ph.D. from M.I.T. in political science in 1963. He is the author of seven books, including *Serious Games* (Viking), *The Evaluation of Social Programs* (Sage), *Applied Research for Social Policy: The U.S. and the Federal Republic of Germany Compared* (Abt Books, Abt, Arrow, Fitzsimmons, eds.), *Perspectives on the Costs and Benefits of Applied Social Research* (Abt Books, Abt, ed.), *Problems in American Social Policy Research* (Abt Books, Abt ed.), *The Social Audit for Management* (AMACOM), and *A Strategy for Terminating a Nuclear War* (Westview Press, 1985).

Clark Abt came to the United States as a seven-year-old refugee immigrant from Nazi Germany. He lives and raises his family in Cambridge, Massachusetts. He and his wife, Wendy, have two children, Thomas and Emily, attending Cambridge schools. Clark Abt has served on the boards of Mt. Auburn Hospital, the Theater Company of Boston, the Poets' Theater of Cambridge, the League School, and visiting committees at M.I.T. and Harvard University. He won the Thoreau Grand Award for landscape architecture for his Abt Research Park design in 1973, and the Cambridge Community Confidence Award in 1978 and 1979. He will be a candidate for Congress in the 8th Congressional District of Massachusetts in the 1986 elections.

STATEMENT OF DR. CLARK C. ABT, PRESIDENT AND CHAIRMAN, ABT ASSOCIATES INC., CAMBRIDGE, MA

Dr. **Abt**. Thank you, sir.

My name is Clark Abt. I am president of Abt Associates, which for the past 20 years has been one of the major firms in the application of social science research to increase the effectiveness and reducing the cost of Government programs in employment, economic development, health, education, income security, housing, environment, public safety, and defense.

Like the majority of my colleagues here this morning, I was trained both as an engineer, physical scientist, and a social scientist; and I resist the distinction that is sometimes made about the relative scientific quality of the two kinds of endeavors.

I welcome this opportunity to summarize my views on particularly the utility of applied social research and development on which I will concentrate. I don't hold the view that all applied social research has been productive or that more Government research funding is needed. I do believe, however, that applied social research has made important contributions to the operations of Government and industry programs and has saved much money by indicating the appropriate elimination or curtailment of ineffectual programs.

I would like to associate myself on my remarks with some of those by my colleagues, Jim Coleman, Joe Newhouse, and Mr. Albers, particularly on the need for data access and a balanced relationship between responsiveness to Government clients versus intellectual independence made by Jim Coleman, the great value and enormous scientific progress made possible by the large-scale management and policy experiments that Dr. Newhouse discussed, and the renewed emphasis by American industry on fuller use of the social sciences to achieve greater productivity and international competitiveness—particularly the quantitative and interdisciplinary social science research activities.

The conclusions of my own review of the productivity of social science research after a 10-year gap between when I last reviewed this for the International Economics Association, is as follows. Then I will try to answer some of these suggested questions and make some brief recommendations.

Social science research and development cannot solve social problems by itself, but it can and does provide information useful to Government decisionmakers in the form of studies, experiments, plans, evaluations, data bases, and analytical tools. By the application of these methods and techniques and informational products, social science R&D can save many more dollars than it consumes; and I have some examples that suggest that we have billions at the research cost of millions—two orders of magnitude.

Successful private industry has been increasing its investment and use of social science R&D, and this suggests one way of increasing productivity of Government-directed social science is by opening it up to more competition. Social sciences are increasingly being used by industries to respond productively to both regulation and deregulation and various kinds of Government and private incentives. The principal industries in which social sciences research and development is finding increasing application are the automobile industry, the health care industry, financial services industry, including banking, communications industry, and the defense industry.

In the aggregate, increasing productivity of social science research does not require increased funding but does justify continuity of funding. The most expensive thing the Government does is to change its mind. Within specific agencies, where evaluation research has been eliminated, I believe it should be restored lest billions be spent on ineffective Government programs for lack of scientific evaluation resulting in reallocation to more productive programs.

The refunding of the missing and urgently required evaluation research in some agencies can be made affordable without an increase in overall budgets by the transfer of funds from other agencies which are less productive in their use of social science R&D. It seems to me essential for deficit reduction—and I assume that we all support that—that the larger spending agencies administering the highest cost programs, such as defense, Social Security, health, housing, and agriculture, be required to execute annual programs, cost-effectiveness evaluations to prune the ineffectual and reinvest in the productive just as are done in our well-managed industries.

Without a steady supply of competent and objective evaluation research results and longer term field experiment results, estimating relative productivity of the Government programs, the Congressional Budget Office and the General Accounting Office and, indeed, the Office of Management and Budget cannot carry out their missions, secondary analysis and rational budget formulation and recommendations. And Congress cannot carry out its mission of more efficient allocation of national resources.

The large-scale continuing data bases that Professor Coleman talked about, and large-scale policy experiments that Dr. Newhouse talked about, are essential for maintaining a continuing and a continually improving set of data bases, and the supply of information to those data bases on which more rational policy decisions can be based and on which we can base the design of more productive Government programs.

I have contented myself with a few recommendations for congressional action that seem to emerge from these conclusions. First of all—and then I will answer the question put to us specifically—in general I would recommend holding the aggregate funding of social sciences research and development stable while increasing social sciences productivity by reallocating from the less to the more productive efforts on the top priority socioeconomic problems: deficit reduction, economic growth, national security, and improved social services.

The problem is in the Government management of applied social research that we tend to spend a disproportionate share of the resources—the research resources—on tinkering with the administration and attempting to refine the administration of the smaller programs such as welfare, and we spend a disproportionately smaller amount on the larger programs which have the greatest potential for productivity improvements.

I would recommend continuing to initiate the top priority crosscutting congressionally mandated studies that have been so productive in the last 20 years, and that only the Congress can mandate because they do cut across agency lines and interests and sometimes across disciplinary lines and are unlikely to get initiated at the agency level without congressional stimulus. It is in the nature of many of our most pressing national problems that no particular Federal agency in a sense owns or takes responsibility for the problems.

I made a brief list of my own judgment of about a dozen most important problems facing the Nation today, and most of these must be addressed by more than one Federal agency. For example, reversing the decline of the American family. Now, with one-fifth of all births illegitimate, that's a figure that many in the public are not aware of and it is a problem that cannot be addressed just in the Health and Human Services Department or in NIMH or in the Labor Department or by the Justice Department or HUD or one of the other agencies. It is a crosscutting issue.

Another one is the prevention of the corruption of the youth by drugs. This is a problem shared by our education agencies, our law enforcement agencies, our health care agencies and is, again, a problem susceptible to crosscutting research.

Those are only a sample. I hope you will continue to invite direct pro bono testimony by the most relevantly-experienced social science researchers on Government-funded social science projects and social programs under consideration, not just at budget time, annually, and concerning the overall budget envelopes, but concerning specific programs and budgets for agency-based social sciences research as well. They know where the bodies are buried.

I would recommend including private industry in the competition for Government social sciences research and development to a greater extent than has been the case in the past to maximize productivity and reduce Government costs. To some extent this trend is already underway, and industry has surpassed Government in the last decade in its funding and support for social sciences research and development.

Finally, to the extent that Public Law 97-34, the Economic Recovery Tax Act of 1981, is going to be reconsidered this fall, we would recommend the exclusion—that we would recommend an end to the exclusion of social science R&D from the R&D tax credits when this public law is being reconsidered. We think that would increase the economic incentives to private associations and have private industry payoffs for social science research of the kind Dr. Albers referred to.

A few specific responses to some of the questions suggested by the committee. Your first question was: Should the Federal Government collect data to describe Federal support for development in behavioral and social sciences? To some extent this is done now by the Congressional Budget Office and the GAO, and I think there are definitional problems here that I don't think are particularly productive to address by a special program.

The second question is: Should industry develop more programs and give more support to training social science professionals? I would let the market continue to decide that issue as to the most productive use of social scientists. I don't think industry needs any special stimulus to make productive use of social sciences, as evidenced by the fact that despite the discrimination against the social sciences in the R&D tax credit, industry has been making much broader and greater use of the social sciences and expanded that use more rapidly than Government.

The question, currently federally collected data are inadequate to describe the amount of industrial behavior and social research. At what level of support does this sector provide? I don't know the answer to that very accurately. My own estimate is it is on the order of $2 to $10 billion depending on definitions. If you want to include all the market research, personnel research, and the business school courses that are essentially business applications of social science methods, and all the education and training which itself has been called a $40 billion industry, the number could be larger than that.

Is there an adequate supply of behavioral and social scientists to meet projected demand? I believe there is, both in industry and in Government.

How does behavioral and social research conducted for the Government by university researchers differ from research performed in think tanks or profitmaking firms? I must confess to some bias on this issue since I am the founder and president of a profitmaking think tank. However, we live in a symbiotic relationship with our university colleagues. They are our principal suppliers of professional staff. They are frequently subcontractors of disciplinarily specialized parts of the work, and we are also dependent on them for long-range basic research which is not funded very heavily by either industry or Government. The main difference is that university research tends to be smaller scale, more basic research oriented, less applied. I believe there is less quality control. That isn't to say that the average level of quality is less, but the heights are probably higher and the depths are probably greater. There is less budget control, and there is less schedule control. It operates a lot more like basic research than production work, as you would expect. Each has its virtues and limitations.

There are many criticisms of behavioral and social research not being used in policy making as it was intended to be. Are these criticisms valid? How can utilization be enhanced? I don't think it's particularly productive to keep complaining to the market for one's work that the market isn't showing sufficient demand. If there is a problem in the demand for more social science research, that social scientists would like to supply, they have a marketing problem and I think the best way to address the marketing problem is to demonstrate utility and effectiveness and economy in the supply and be very competitive with the internal sources of research. Senator Brooke once asked me when he was still a senator why the Federal Government should support social sciences research outside the Federal Government. Why it shouldn't all be done within the Government? And I told him there were three reasons: One was that if we didn't do a good job we could be fired, unlike civil servants; second, we were capable of recruiting and mobilizing a great many very bright and capable researchers who for one reason or another would be unwilling to work for the Federal Government directly; and the third reason was that the need and demand for specific kinds of social science research fluctuated greatly and there was no reason why the Government should bear the extra overhead costs of having to maintain a large standby capacity, a large inventory of social science researchers when the demand for specific kinds of work fluctuated continually.

Mr. WALGREN. Let me stop you there, if I might, and I apologize in the interests of time. We got a late start this morning because of my lateness, and I am concerned that we are going to have a vote here relatively directly, and I don't know about our ability to come back after that time at the hiatus. If there is anything further you would like to just pinpoint, Dr. Abt, I don't mean to cut you off and I apologize for that, but I did want to take some time while Mr. Slaugther and Mr. Packard are here to give them an opportunity to raise some thoughts before we all got called away by a vote that may come relatively soon.

Mr. ABT. One final point in answer to your question 16: What should be the criteria for Federal support of research in the social and behavioral sciences? Research support, I think the main criteria should be relevance, validity and productivity in terms of their effect on programs, Government program effectiveness, equity and efficiency.

[The prepared statement of Dr. Abt follows:]

IMPROVING THE EFFICIENCY OF GOVERNMENT WITH SOCIAL SCIENCE RESEARCH

Clark C. Abt, Ph.D.
President and Chairman
Abt Associates Inc.

I am Clark Abt, President of Abt Associates Inc. For the past twenty years, my firm has been a leader in the application of social science research to increasing the effectiveness and reducing the costs of government programs in employment, economic development, health, education, income security, housing, environment, public safety, and defense. I welcome this opportunity to summarize my views on the utility of applied social research. I do not hold the view that all applied social research has been productive or that more government research funding is needed. I do believe, however, that applied social research has made important contributions to the operation of government programs and has saved much money by indicating the elimination or curtailment of ineffective programs.

Utilization Trends of the Past 25 Years

Since 1960, the applications and uses of social sciences R & D by government and industry have undergone significant changes in both the volume and distribution of activities, in both government and industry. In government, the early 1960s saw much expanded use of social science R & D by the Departent of Defense and the Office of Economic Opportunity in its anti-poverty activities. Most of the work was requirements research, such as needs surveys, program planning studies, and computer modeling for planning and assessment. The Rand Corporation probably played the early leading role in the 1960s in mobilizing the social sciences — particularly economics, applied mathematics, political science, and psychology -- in the service of government program policy research and planning. In the last half of the 1960s, disenchantment with Project Camelot-related defense applications coincided with a great expansion of the War on Poverty programs. The principal source of support for government social sciences R & D shifted from DOD to the civilian agencies, and the market expanded for broader and more anti-poverty and equity-oriented researches. With the Nixon Administration and the early 1970s came an increasing concern for the evaluation and selective weeding of the Great Society programs of the Johnson Administration, and here ironically social sciences R & D saw its greatest historical rate of expansion. Emphasis shifted from research and planning to program evaluation and the development of data bases and measurement tools for more effective program audits. The 1970s marked a major methodological advance from small and often qualitative studies to more scientific social policy experiments, particularly in employment, income security, education, housing, and child and health care. In the late 1970s, emphasis tended to shift towards the more economically significant program evaluation and research areas in response to increasing cost concerns. By the early 1980s, utilization emphasis had clearly shifted from education, equalization of opportunities, employment, housing and welfare to health and income security.

In industry, the early 1960s saw the expansion of educational technology with programmed instruction and later in the 1970s, the introduction of microcomputers for instructional purposes. By the middle 1960s, the management consulting industry was beginning to adopt operations research and computer modeling methods from what had been primarily Defense Department applications. Political polling blossomed in the early

1960s with the expansion of quantitative voting behavior studies by political scientists working with survey sociologists, and increasingly sophisticated sampling methods devised by statisticians and mathematical sociologists. Market research and product evaluation were also slowly being improved in the 1960s by the transfer of some of the social sciences R&D developed for the government. In strategic planning by the late 1960s, the cycling of military cost-effectiveness analysis and 'systems analysis' came full circle by way of Mr. McNamara and many others from industrial engineering to the business schools to industrial management to the Department of Defense to the civilian agencies of government, and back to private industry. Most business school curricula incorporated much of the social sciences R&D methods and findings.

By the 1970s, the survey research industry had expanded in both size and quality through its investment in the large-scale national surveys and associated government social sciences research. By the 1980s, market research (particularly in the pharmaceutical, health products, automobile and consumer products industries) had made substantial methodological progress. Firms were designing and operating sophisticated market experiments and evaluations approaching the quality of government social policy experiments, and frequently at lower cost.

Spending Trends of the Past Twenty Years

Measured in terms of constant purchasing power, federal spending for applied social science research fluctuated sharply at the start and end of the 1970s. Real spending rose by nearly 40 percent between the late 1960s and early 1970s, and then remained relatively constant until 1980. Sharp reductions in spending in the early 1980s returned us to the spending levels of the late 1960s.

Most of the reduction in federal spending for applied research in recent years occurred in the Departments of Health and Human Services, Labor, and Education. Between 1979 and 1985, real spending for applied social research in the Department of Defense rose, by more than one-third. Relative to GNP and total federal outlays, spending for applied research has fallen steadily since 1978.

Utility of Research to Government Operations: Three Examples From Housing, Education, and Health

Of the $7 billion spent by the federal government on applied social sciences research over the past twenty years, not all was used productively. The large increase in spending in the late 1960s and early 1970s could not have occurred without some loss in research productivity, for no industry can expand so rapidly without some inefficiency. Similarly, it is likely that the reductions in research funds in the early 1970s were accompanied by an increase in research productivity, as marginal projects were eliminated and less productive researchers lost their federal support.

Despite the occasional ill-conceived and poorly-implemented research project, applied research has made many important positive net contributions to government operations. The contributions are particularly important in our current economic circumstances. The serious threat of continuing and growing government deficits and the reluctance of the public to support tax increases apply increasing pressure to hold overall spending on social and economic programs relatively fixed, while responding to increases in demand with improved productivity of supply. The only way the public's demand for more, better, and more broadly distributed government services can be met within fixed budgets is by increased productivity of these government activities.

Government investment in policy and program research and evaluation needs to yield only a very small improvement in government programs to produce an enormous social return. Spending for applied social sciences research during the past two decades has consumed only 8¢ out of every $100 of the budgets of federal agencies. If this small level of spending on research improved the effectiveness of the remainder of federal budgets by only one percent, it produced in excess of a tenfold return.

I cannot give you a precise quantification of the total, average, or marginal rate of return to applied research, but I can cite some important partly monetized examples of the contributions of research to the cost-effective operation of government programs.

An Example From the Housing Allowance Demand Experiment

The Housing Allowance Demand Experiment, together with the Housing Allowance Administrative Agency Experiment and the Housing Allowance Supply Experiment, were key steps in the evolution of a policy focus on low-income housing assistance programs based on the use of existing, privately-owned housing whose ramifications continue to be developed to this day. This was not a new idea generated by the experiments, but its confirmation by the experiments has clearly dominated subsequent housing policy.

The Demand Experiment had three major findings.

1. The incidence of physically inadequate housing among low-income households was found to be much higher than any previous estimates (43 percent versus 13 percent).

2. Housing programs have a much larger impact on the housing conditions of recipients than comparable income maintenance. The two surprising parts of this finding were, first, that housing change under income maintenance transfers was less than half the figure commonly accepted before the experiment and, second, that there was a sharp tradeoff in program design between the extent to which the program would lead to physically improved housing and the extent of participation.

3. New construction program costs were considerably higher than the costs of programs based on use of the existing housing stock. In terms of the programs now in effect, the findings were that conventional public housing would cost roughly 50 percent more than a program like the current Section 8 Existing Housing program for the same housing.

The experiment more than paid for itself. The Housing Allowance Demand Experiment cost roughly $30 million. The use of existing housing has saved hundreds of times this amount. In Fiscal Year 1983, for example, the Section 8 existing budget was about $4.4 billion. Given the relative cost findings (item 3 above), this is $2.2 billion less than the cost of similar conventional public housing for that one fiscal year. No one can claim that this decision rested solely on the Housing Allowance Demand Experiment, but the Experiment provided the first convincing evidence of the actual magnitude of the effect. Even if only 10 percent of the decision rested on this social policy experiment, it was clearly worthwhile.

An Example From Early Childhood Education: The National Day Care Study

During the late 1960s and early 1970s, federal day care regulations imposed relatively stringent staffing requirements on day care centers in an attempt to insure an enriched environment for preschool children from poor families. The National Day Care Study, conducted at a cost of less than $10 million, demonstrated unequivocally that, so long as group size was not large, staff/child ratios could be reduced without a reduction in quality of care. Ensuing relaxation of federal staffing requirements freed more than $60 million a year in federal and state day care funds for services to additional children, and likely produced a sizeable savings in parent fees as well.

An Example From Health Care: Hospital Prospective Reimbursement Studies

Until 1983, hospitals were reimbursed by Medicare, Medicaid and Blue Cross plans in most states on the basis of actual incurred costs. The absence of an effective budget constraint was a major cause of hospital costs rising during most of the period from 1966 to 1983 at a rate about 50 percent faster than the annual rate of increase of the cost of hospital inputs. Starting in the late 1960s, a number of states began to experiment with an alternative form of hospital reimbursement, one by which the rate of payment for hospital services was set prospectively. Federally sponsored studies of these experimental programs carried out at a cost of less than $10 million, indicated that the alternative reimbursement approach reduced the annual rate of increase of hospitals cost by 2 to 4 percentage points per year and achieved savings measured in billions of dollars per year. Encouraged by the results of these evaluations, Congress and the Health Care Financing Administration implemented such a program for Medicare on a national basis in October 1983.

Although my alloted time does not permit me to provide additional illustrations in this presentation, I have included a number of other examples in the addendum to my prepared text.

Level and Allocation of Federal Social Sciences Research Funds

Despite the recent reductions in real federal spending for applied social science research, I do not argue that funding for applied research be significantly increased. Much of the relatively high spending on applied research in the 1970s could be justified as an effort to assess the efficacy of the multitude of relatively new programs. With the end of the expansionary phase of federal spending, and with strict constraints on the budget, we must focus applied research on potential improvements in operating efficiency.

Arguing against major increases in funding of social science research are the observations that there is close to full employment among social scientists and social researchers, and dramatic increases in funding are likely to create salary inflation and productivity declines. One cannot rapidly expand the supply of competent social scientists by readily expanding budgets. Furthermore, there is a sufficient fraction of unproductive research projects, so that merely thorough and periodic review of research productivity is very likely to identify enough misapplied resources that could be more productive reallocated to the increased investment in the more productive social science research projects.

There are some important emerging opportunities for applied research improving the productivity of government policies and programs. These include:

- mechanisms for reducing reliance on expensive nursing home care for the elderly and better funding and targeting of the often-preferred home-based care alternatives;
- increased conventional deterrence by increased crisis availability and readiness of military manpower through expanded and more effective recruitment, training, and deployment of reserve forces;
- the efficacy of vouchers as an alternative mechanism for funding public education;
- cost-effectiveness of privatization of public housing, military health care facilities, and other public functions;
- rebasing of military facilities to achieve force multipliers and economies.

Getting More Productive Government Social Sciences Research in the Service of More Productive Social Programs Without Budget Increases

The issue is the utilization of the social and behavioral sciences in government and industry, and particularly the impact of social sciences research (including economics, survey sociology, statistics, psychology and political science) on the effectiveness, equity and efficiency of government policies and programs. The three policy issues I want to address are 1) Should we spend more, less or the same on government, social and economic programs and the research on which they are based or by which they are evaluated? 2) Whether we spend more, less or the same on the program research and the programs, how should we change the mix of expenditures? and 3) How do we decide how to redirect our expenditures in a more productive direction?

Our general finding concerning the level of spending is that, for the last twenty years, the average level of expenditure on social and behavioral research has been roughly constant, ranging between $300-450 million in constant 1972 dollars, although there were sharp increases in expenditures in the late 1960s under the Nixon Administration and again both sharp annual increases and succeeding decreases in the Carter Administration in the late 1970s, followed by further sharp annual decreases and then increases in the Reagan Administration. We have no conclusive evidence that the relatively short-term one- or two-year increases and decreases had strong impacts on research productivity or effectiveness. There is also no clear evidence supporting claims for substantially increasing or decreasing the aggregate level of spending that has been held relatively constant ± 20 percent over the last twenty years.

The variation in the level of spending on the programs themselves, as distinguished from the social and behavioral sciences research, has steadily increased in total cost and also in total impact over the past twenty years from 1965-1985. This would imply a steady and significant twenty-year improvement in the productivity of applied social sciences research as the R & D basis of the programs, because the research share of the total programs expenditures has steadily shrunken, while slow but steady progress has been made in the validity, relevance and overall intellectual rigor and management efficiency of the research. (Evidence for the steady twenty-year increases in research efficiency can be gathered from the much lower rate of research cost growth, relatively tiny rate of research contract overruns, and much more modest research labor cost growth than in the rest of government R & D and the economy at large -- most social scientists work hard, well and cheap.)

Concerning the spending level of social and economic programs as distinguished from research about them, given the very serious threat to our economy of continuing and growing government deficits and the reluctance of the public to support tax increases or give up tax cuts, we see no alternative but to hold overall spending on social and economic programs relatively fixed, despite ever-increasing qualitative and quantitative demands and participation rates. Thus, the only way that the public's demand for more, better, and more broadly distributed government programs can be met within fixed budgets is by increasing the productivity of government programs and services. Such productivity increases have been steady, significant and widespread in business and industry since the birth of the nation, and can and should be required of government as well. The fastest, cheapest and most effective means of achieving such government productivity increases is to invest in the appropriate kinds and amounts of government policy research carried out by social and behavioral scientists with particular emphasis on the benefit-cost analyses of economists, the needs surveys and preference estimates of the social psychologists, and the policy experiments designed by the statisticians and other social scientists.

Concerning the third issue of how to identify the most productive researches and programs to which to reallocate investment, we recommend the creation of data bases encompassing time series of measures of productivity of both research and programs. The best measures of research productivity are those that measure validity, relevance, and efficiency of research. The best measure of program productivity can measure effectiveness, equity, and efficiency. There is substantial agreement in the social sciences community about many of these measures of both programmatic and research productivity. We recommend more consistent, continuing and comprehensive collection of these measures and the application of these measures on at least an annual basis to reallocate both research and program resources to the more productive from the least productive within essentially constant budget envelopes.

Productive Response at the Enterprise Level

The organization I founded twenty years ago, Abt Associates Inc., may serve as a representative example of how the larger and more successful private profit-making, employee-owned social science policy research firm can survive and thrive in a competitive and budget constrained environment. We started in a mixture of defense and economic development and educational researches in the middle 1960s, had a major expansion with the War on Poverty activities of the OEO and contraction in our military and private education research, and then the great expansion with the large scale social policy experiments of the 1970s. This was followed by the decline of support for both the policy experiments and the overall government social sciences R & D market in the late Carter and early Reagan Administrations (1978-1982). Abt Associates Inc. survived its first major cut-back and reduction in forces in 1980-1982 as a result, shrinking from a staff of over 1000 (of whom half were social science professionals with advanced degrees) to one-third that size.

Abt Associates Inc., in the middle 1980s, recouped to half its peak size by diversifying into the private sector and finding many new opportunities for transfer of government social sciences R & D technology to private sector work in electronic banking, automotive market planning, epidemiology for pharmaceutical companies and economic health benefits planning for employers and health care institutions, as well as general corporate strategic planning, market research, and personnel and marketing program evaluation. This transformation, from a private firm almost entirely a government social policy research contractor to a mixture of public policy research and private consulting, could not be accomplished without the loss of some talent.

However, our most productive and competent social scientists have generally found full employment, either inside the company in the application of social sciences R & D to the private sector and the now more slowly growing public sector social sciences R & D activities, or outside the company in private industry or academic careers. We are not aware of a significant number of competent and dedicated social scientists who have been forced to leave the field. This suggests that the current shift in the funding of the American social sciences R & D establishment from predominantly government and universities, and to a lesser extent foundations, to a still predominantly government and much increasing private sector is an opportunity and a trend we have been fortunate in anticipating and should strengthen and broaden.

It is for particularly this reason that we should make better industry use of the uniquely competent, diversified, and generally superior American social sciences R & D community, that the Economic Recovery Tax Act 1981, PL97-34, which explicitly excluded social policy research from R & D tax credits, should be reconsidered. The Congress, when examining the Act due to expire this December, 1985, may want to consider amending it to include behavioral and social sciences R & D tax credits, so as to further facilitate the privatization and technology transfer of social sciences R & D from primarily government-funded research to a more balanced combination of government and private support.

CONCLUSIONS

Congress and the social scientists need to develop a common understanding concerning what government social science research can and cannot do, based on the actual results of the last fifty years. Social science R&D can provide thorough and objective information to government decision makers, including studies, analyses, action plans, evaluations, and knowledge products such as data bases and tools for testing and measurement. Notable achievements among data bases are the National Income Accounts created by economists and statisticians in the 1930s, several major nationally standardized educational achievement tests (created by educational psychologists and statisticians), and the Current Population Survey.

Social Science R&D cannot "solve" social problems, any more than physics research can solve defense problems. Congress and the social scientists both can develop a common understanding and division of labor concerning the information, analysis, planning and testing and evaluation services that social science R&D is alone capable of providing to government decision makers, and the corresponding legislative, executive, and judicial branch decision-making that can be improved by social science R&D inputs.

What the Congress can and cannot do concerning use of and support for social science R&D also needs to be better understood by both communities. The Congress can and should ask the most important national economic and social policy questions of the social science R&D community, including particularly those questions which do not fit into single disciplinary niches or Federal agency missions. Social scientists should meet the market for ideas in Congress rather than complaining of Congress's insufficient utilization or support for its scientific and information services. The Congress cannot and should not try to regulate and micromanage how the questions it asks of the social science R&D community are answered.

Government social science R&D is undertasked on our most important social and economic problems, yet overcommitted to the more marginal issues of social programs administration. One selection of the top dozen problems facing the United States government, concerning which decisions could be improved by social science R&D focused on them, is:

1. Deficit reduction integrated with fairer and more productive taxes
2. Better, more widely-shared and more affordable health care
3. Reversing the decline of the American family, now with one-fifth of all births illegitimate
4. Preventing the corruption of the youth by drugs
5. Improved educational productivity
6. The social and economic security of the elderly
7. Countermeasures and deterrents to international terrorism and its threatening to become nuclear
8. World poverty and starvation, and its fomenting of revolutionary conflicts
9. United States maintenance of deterrence of nuclear war and aggression, and enhanced reassurance of allies and domestic publics
10. Improved United States international industrial competitiveness
11. Continuation of United States world scientific and technological leadership
12. Continuation of United States world moral leadership supporting justice, peace, prosperity, and freedom for all nations

The top twelve corresponding social science R&D investments appear to be less than ideally matched to these national and global priorities of much greater human magnitude. For example, most of the Department of Defense's social sciences R&D is invested in military tactical training rather than the economics of improving defense productivity or the psycho-economics of nuclear deterrence. This is not to argue that DOD current social sciences investments are unproductive, but only that the most urgent problems are underincented in comparison with more routine ones.

Congress has a unique opportunity to mandate important cross-cutting social sciences R&D, which is not part of any single agency's or discipline's mission, without going to the risky expense of setting up new agencies. The cross-cutting social sciences R&D opportunities include military deterrence (interested agencies: DOD, NIMH, DOS/AID), family decline (HHS, DOL, NIMH, DOJ, DOD, HUD, DOA), industrial productivity competitiveness (DOL, DOC, DOD, DOA, DOT, TREAS, DOS, HHS, EPA, DOI), taxes (TREAS, OMB, HHS,DOL, DOC, DOI), educational quality (HHS, DOL, HUD, DOD), crime (DOI, HUD, HHS), and transportation (DOT, EPA, HUD, DOD, DOI).

Only Congress can decide a better division of labor of government social sciences R&D efforts among public (CBO, GAO, FCRCs) and private agencies (universities, industry, foundations) and assure that an open competitive market exists that will motivate the best work.

More open competition, not more regulation, is needed to increase the productivity of U.S. social sciences R&D. Let the marketplace of ideas, not regulation, decide whether basic or applied, public or private sector, university or industry or FCRCs are the most cost-effective suppliers of government social sciences R&D demand, which will avoid cost inflation, stagnant monopolies in knowledge specialties, and too narrow or superficial thought.

CONCLUSIONS

- SOCIAL SCIENCE R&D CANNOT "SOLVE" SOCIAL PROBLEMS, BUT CAN AND DOES PROVIDE INFORMATION USEFUL TO GOVERNMENT DECISION MAKERS, IN THE FORM OF STUDIES, PLANS, EVALUATIONS, DATA BASES, AND ANALYSIS TOOLS.

- SOCIAL SCIENCE R&D CAN SAVE MANY MORE DOLLARS THAN IT CONSUMES (Saves billion for research cost of millions)

- INCREASING PRIVATE INDUSTRY INVESTMENT IN AND USE OF SOCIAL SCIENCE R&D SUGGESTS INCREASING PRODUCTIVITY OF GOVERNMENT-DIRECTED SOCIAL SCIENCE R&D BY INCREASING COMPETITION (Social sciences are increasingly used by industries to respond productively to both regulation and deregulation and incentives.)

- IN THE AGGREGATE, INCREASING PRODUCTIVITY OF SOCIAL SCIENCE R&D DOES NOT REQUIRE INCREASED FUNDING, BUT DOES JUSTIFY CONTINUED CONTINUITY OF FUNDING (THE MOST EXPENSIVE THING THE GOVERNMENT DOES IS CHANGE ITS MIND.) BUT WITHIN SPECIFIC AGENCIES, WHERE EVALUATION RESEARCH PARTICULARLY HAS BEEN ELIMINATED, IT SHOULD BE RESTORED, LEST BILLIONS BE SPENT ON INEFFECTIVE PROGRAMS. THIS REFUNDING OF MISSING URGENTLY REQUIRED RESEARCH IN SOME AGENCIES SHOULD BE MADE AFFORDABLE BY TRANSFER OF FUNDS FROM OTHER AGENCIES WHO ARE LESS UNPRODUCTIVE. IT IS ESSENTIAL FOR DEFICIT REDUCTION THAT THE LARGER SPENDING AGENCIES, ADMINISTERING THE HIGHEST COST PROGRAMS SUCH AS DEFENSE, SOCIAL SECURITY, HEALTH, HOUSING, AND AGRICULTURE, BE REQUIRED TO EXECUTE ANNUAL PROGRAM COST-EFFECTIVENESS EVALUATIONS TO PRUNE THE INEFFECTUAL AND REINVEST IN THE PRODUCTIVE, JUST AS IS DONE IN WELL-MANAGED INDUSTRIES.

- WITHOUT A STEADY SUPPLY OF COMPETENT AND OBJECTIVE EVALUATION RESEARCH RESULTS, ESTIMATING THE RELATIVE PRODUCTIVITY OF GOVERNMENT PROGRAMS, CBO AND GAO, CANNOT CARRY OUT THEIR MISSIONS OF SECONDARY ANALYSIS AND EVALUATION, AND CONGRESS CANNOT CARRY OUT ITS MISSION OF MORE EFFICIENT ALLOCATION OF NATIONAL RESOURCES.

RECOMMENDED GOALS FOR CONGRESSIONAL ACTION ON GOVERNMENT SOCIAL SCIENCES R&D

1. TOP-PRIORITY, CROSS-CUT CONGRESSIONALLY MANDATED STUDIES

2. REQUIRE DIRECT PRO BONO TESTIMONY BY MOST RELEVANTLY EXPERIENCED SOCIAL SCIENCE RESEARCHERS ON GOVERNMENT-FUNDED SOCIAL SCIENCE R&D RESEARCH PROJECTS

3. END THE EXCLUSION OF PRIVATE INDUSTRY FROM COMPETITION FOR GOVERNMENT SOCIAL SCIENCES R&D
(Examples: DOA, NSF, etc. etc.)

4. INCREASE ECONOMIC INCENTIVES TO PRIVATIZATION AND INDUSTRY PRODUCTIVITY PAYOFFS OF SOCIAL SCIENCES R&D, BY ENDING EXCLUSION OF SOCIAL SCIENCES R&D FROM R&D CREDIT IN RECONSIDERING THE ECONOMIC RECOVERY TAX ACT OF 1981, P.L.97-34.

5. HOLD AGGREGATE FUNDING STABLE WHILE INCREASING SOCIAL SCIENCES PRODUCTIVITY BY REALLOCATING FROM LESS TO MORE PRODUCTIVE EFFORTS ON THE TOP PRIORITY SOCIO-ECONOMIC PROBLEMS OF DEFINITE REDUCTIONS, ECONOMIC GROWTH, NATIONAL SECURITY, AND BETTER SOCIAL SERVICES.

ADDENDUM

I. CHARTS

1. Total Federal Expenditures for Applied Social Science Research (1972 dollars)

2. Federal Expenditures for Applied Social Science Research as a Percentage of Total Outlays, by Agency (HEW, DOT, DOD, DOL)

3. Federal Expenditures for Applied Social Science Research as a Percentage of Total Outlays, by Agency (USDA, HUD, DOE, DOJ)

4. Federal Expenditures for Applied Social Science Research, by Agency (1972 dollars) (HEW, HUD, DOL, DOJ)

5. Federal Expenditures for Applied Social Science Research, by Agency (1972 dollars), (USDA, DOT, DOD, DOC)

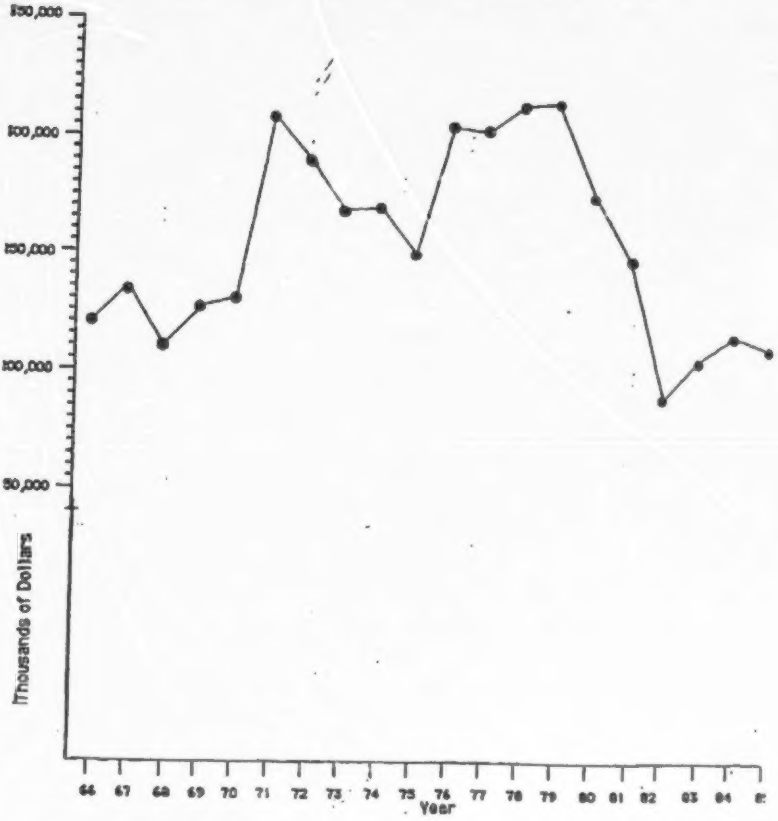
6. Federal Expenditures for Applied Social Science Research, by Agency (selected years, current dollars)

II. BACKGROUND MATERIAL - CLARK C. ABT

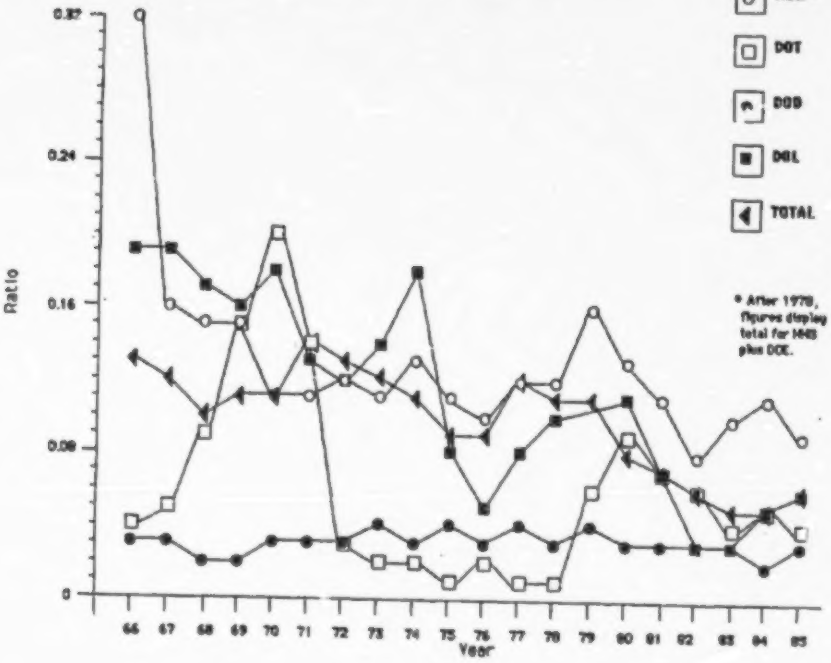
1. Short Biography

2. Vita

Total Federal Expenditures for Applied Social Science Research (1972 dollars)

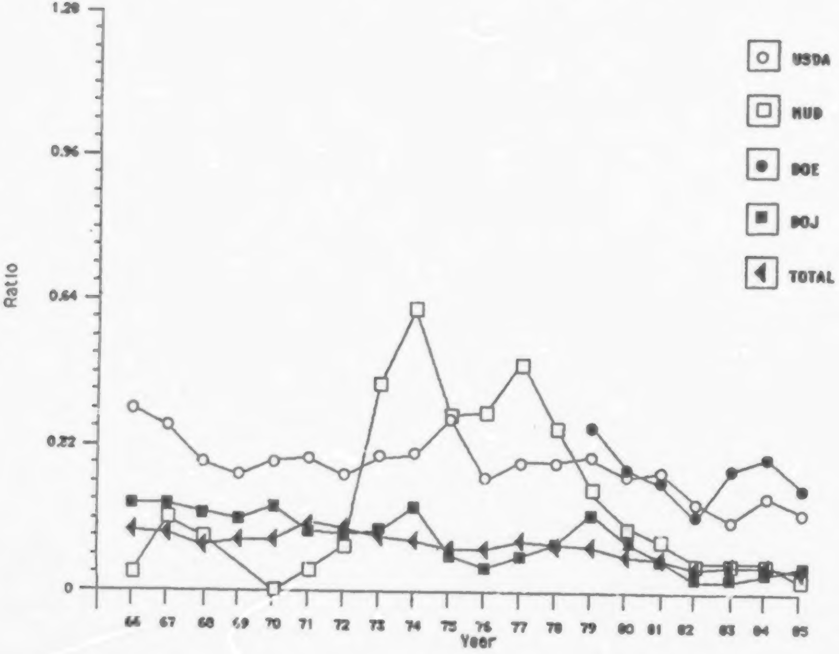


Federal Expenditures for Applied Social Science Research as a Percentage of Total Outlays, by Agency

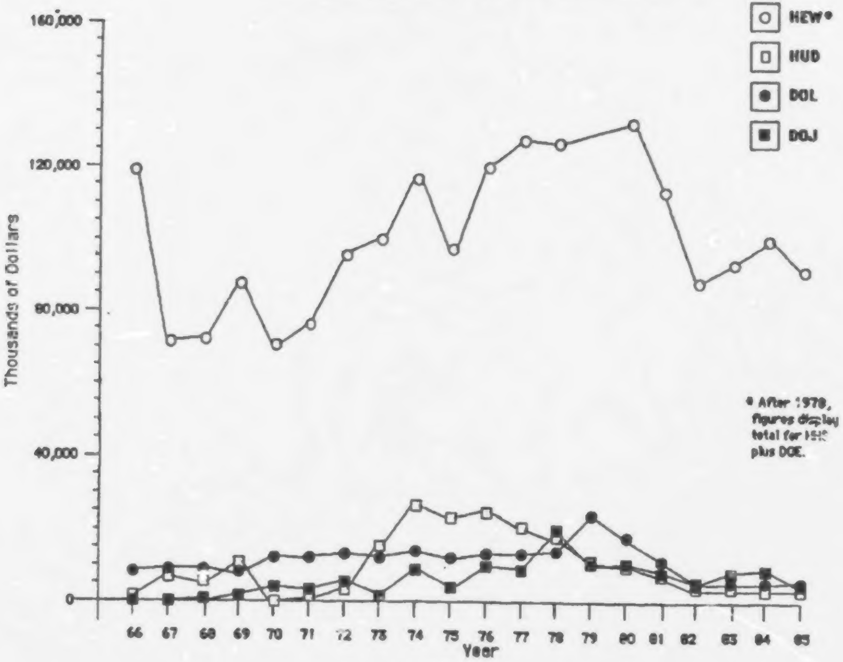


* After 1979, figures display total for HHS plus DOE.

Federal Expenditures for Applied Social Science Research
as a Percentage of Total Outlays, by Agency

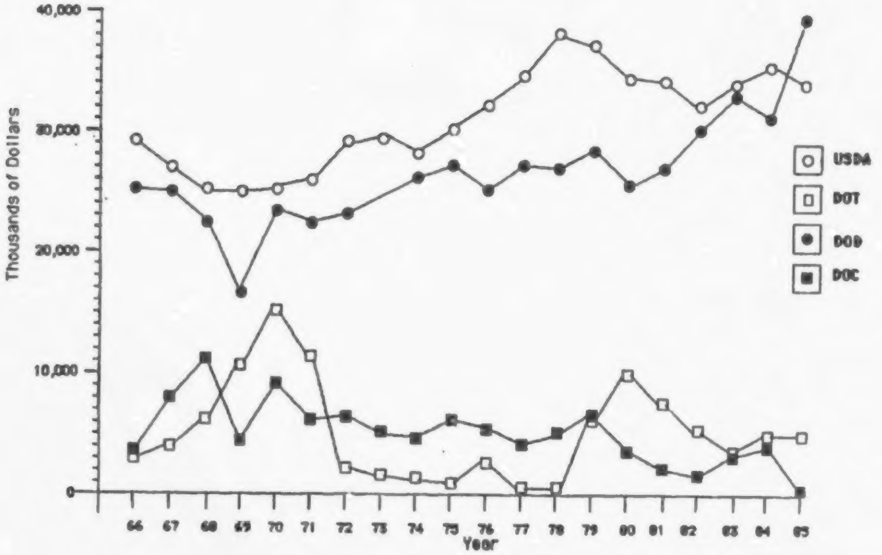


Federal Expenditures for Applied Social Science Research, by Agency (1972 dollars)

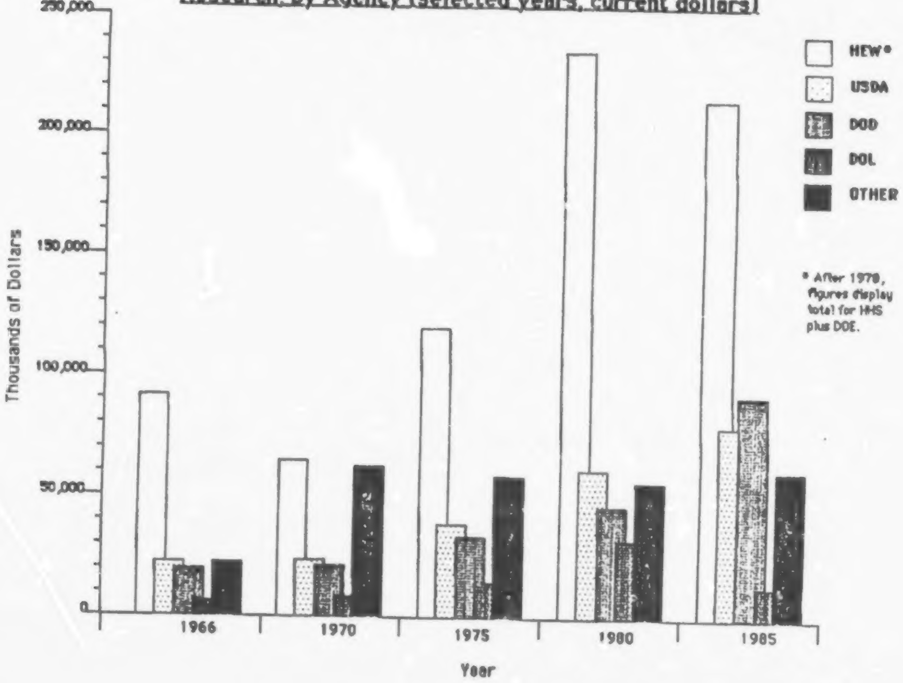


* After 1978, figures display total for HHS plus DOE.

Federal Expenditures for Applied Social Science Research, by Agency (1972 dollars)



Federal Expenditures for Applied Social Science Research, by Agency (selected years, current dollars)



* After 1978, figures display total for HHS plus DOE.

DISCUSSION

Mr. **Walgren**. C-Span misses some good stuff, and the privilege of sitting back and listening to all of you gentleman is real, and I certainly appreciate your coming before us.

I wanted to ask or fish for more reactions to one thought that is occurring in my mind. That is, you have seen the last 20 years of social science, contributions both governmental and by private industry, and you also have seen a period of great advance in physics and developments in the physical sciences.

Would it be wrong to say that projecting what might happen, what might be contributed in the way of economic gain, advantage of opportunity, in the next 10 years or future range would be as great in the social sciences as it would be in the physical sciences? The reason I ask that question is that by and large we tend to turn away from the social sciences. As you all know, it has been an area where they have distributed Golden Fleece Awards and things like that, and it has been largely unappreciated in some sectors of our society. And yet I can't help but be struck by the concepts of productivity and organizational productivity, motivation, let alone the social problems of criminal difficulties and the like. And I would like to ask just as, I guess, an expression of the value of the area, would you, after giving individual instances of utilization and effective potential, would you encourage us further to appreciate that potential, to say that it really may be as important as, in economic terms, the advances in research and the like that we see on the physical side? And it would be fair to say that by overstating the case you can say that, but I am fishing for some kind of evaluation of overall worth after we have looked at these individual instances of concrete benefit.

Dr. Coleman.

Dr. **Coleman**. I would like to respond to that with simply one example. I would certainly agree with conclusions you have made. Let me give an example from Walter Albers' company where I was on the science advisory committee for 11 years. There were—I was the only social science member of that committee, and this committee was set up by General Motors in the early 1970's to advise the company on what new directions in basic research in science, in natural sciences, should not be—not just in basic but in research and development that would help General Motors' productivity. As it turned out, most of the problems that the committee identified ended up being problems where they originated as a problem of engine design or a problem of quality control or whatever, ended up being problems in the structure of the organization. That is, the technical knowledge was already existing in the organization. For example, when the organization began to look seriously at the problem of becoming competitive with the Japanese, nearly every problem as identified by the committee as a problem turned out to be a problem of incentives in the structure of the organization; that is, a specific comparison was made in the Japanese engine, and technical differences were found. It turned out the technical knowledge clearly existed in the organization. The question was why wasn't it utilized and that was an organizational problem, a social science problem. So that I think that is an example to illustrate exactly that point.

Mr. **Walgren**. Other thoughts?

Dr. Albers.

Dr. **Albers**. I think the answer to your question is a very difficult one because of—I don't mean to be evasive, but there is a question of measurement. Social science research usually results in some actions being taken to defer costs, but those costs aren't measurable because you deferred them. And to try and estimate the magnitude of the contribution of the social and behavioral sciences as compared to the physical sciences where you have a tangible process, a product that comes out, that adds some value to some aspect of the society——

Mr. **Walgren**. By deferred cost, you mean something related to future opportunity lost?

Dr. **Albers**. If one doesn't have the benchmark to measure it from and have the actual observations to measure it from, one can't measure it. So you are talking about an entity, a quantity that is immeasurable. If those of us feel—some of us feel that those costs savings or gains from social and behavior science research, although unestimable, are huge and quite comparable to the value added to society by physical and natural sciences.

Mr. **Walgren**. Dr. Bray.

Dr. **Bray**. I don't think I can really answer the question. Probably nobody else can really. But I do feel that the economic returns from more basic research and then the application of that basic knowledge into organizations in terms of improving their competitive status, their productivity, their quality and all things we might talk about, and well being of the employees, I think would be extremely large.

Dr. **Newhouse**. I would like to distinguish between what I will call the sum or the total benefit cost in the physical sciences and the social sciences. I don't know how to make the benefits commensurate either. But in the health area, which I am most familiar with, the NIH budget, as I recall, for medical research is something on the order of $3 billion, and the health services research budget in Public Health Service and Health Care Financing Administration put together is on the order of $60 million. The relevant question is if a dollar were taken from one place and put in another place, what would the yield be? And it's certainly plausible to me, although I can't prove it and reasonable people can differ with that disparity in magnitude, there would be a difference in the usefulness of the reallocated dollar.

Mr. **Walgren**. This is really Dr. Abt's point.

Dr. Abt.

Dr. **Abt**. Yes. Although if the thrust of your question is if the marginal productivity of social research is greater than physical sciences research, shouldn't we shift funds from physical sciences to social sciences, I would have to answer no. I think American social science research is the world leader clearly, particularly in quantitative, empirical oriented social science research. We are fully invested. There isn't a lot of unemployment. There is plenty of good work to do. But at a funding level of perhaps $1 billion a year roughly from Government and two or three times that from the private sector, it's difficult to see that further Government investment would do most for our economy when in engineering and production management we face very severe international competition. We are also world leaders in the physical sciences, but I believe our lead there is much less secure and shrinking compared to our lead in the social sciences which continues to grow.

So if the thrust of the question is, should the allocation of resources between physical and social science be shifted toward the social sciences in the interests of overall national economic growth, I would disagree and I think we need greater investment in the physical sciences, greater expansion of investment in the physical sciences, engineering and production management. And I think we can expect, and especially if you will demand it, increasing productivity and utility from the social sciences without increases in funding.

Mr. WALGREN. Let me turn to other members who have been kind enough to come and listen.

First recognition will be Mr. Packard.

Mr. PACKARD. Thank you, Mr. Chairman.

I was as not as much amused as I was interested in the comment that Dr. Abt made. I think where he said the greatest cost of Government is its changing its mind. Perhaps as we talk about large-scale research projects—that would be a good project—to determine what it has cost the American people as we changed our minds repeatedly and what system could be developed that would help us to avoid having to change our minds so often.

I am particularly interested in some of the health implications of this hearing and particularly, Dr. Newhouse, your experience and background. Waste and abuse of Government funds and services, as you testified, increases with the costs or actually decreases with the cost of, or the sharing of, those costs by patients or by individuals. As we know, the high cost of health care today has reached almost crisis proportions in this country. Has there been, or is there, a need for a large-scale research project to find a balance between an appropriate level for patient cost sharing versus ongoing adequate health care because of the high cost to the individual and to the patient? Is there—has there been research done on that and, if not, what kind of needs are there in that area?

Dr. NEWHOUSE. That is largely the project I was, in fact, describing. It asked those questions. One important finding, however, that came out of it that was perhaps obvious but only in hindsight is that cost-sharing by patients is not a very effective instrument for addressing the problem of increasing hospital costs which have been the most rapidly rising costs, and the reason is fairly simple, that it is—without asking people to face large financial risk that they naturally don't want to pay, it's impossible to have very much cost-sharing for people in the hospital.

Mr. PACKARD. Let me ask one other question, Dr. Albers. You gave two examples: noise abatement and seat belts. How much of the cost of American products can be attributed to the research that is required by corporate America to address environmental and safety concerns that foreign competition often does not have to address simply because those concerns are not at the levels they are here in the United States? Have there been any projects or research done to determine the additional cost of American products to the American consumers that foreign competition is not required to make in terms of research on environmental and safety and other needs of our products?

Dr. **Albers**. I don't know—I am at least not aware of any research that has been done on sort of the marginal difference between R&D costs of a domestic manufacturer versus a foreign manufacturer. I am talking about the R&D costs. We do understand the difference in manufacturing costs. That sounds like an interesting experiment, set of observations to carry out an interesting analysis. I cannot give you an answer.

Mr. **Packard**. I would assume that imported foreign products have to meet most of the same regulations that our own manufacturing does. But where they sell in their own country, as we visit other countries, we see their standards are considerably different. Noise abatement is a good illustration. We will go to some cities, like Bangkok, where noise abatement is almost totally absent, and their products simply would not sell in America. We have to address those from our own domestic sales. They don't, and the competition, of course, would make it more——

Dr. **Albers**. I certainly have no disagreement with what you're saying. The R&D that is done in the industry I know best, which is the automotive industry, by foreign competitors is small by American standards. But some of the reason for that is that the technology of the automobile has been pretty much developed by the Western society, and our competitors have, in getting entry into that industry, had a broad base of R&D to work from to start with, so the marginal R&D they have to do is sort of relatively less costly than the large amount of R&D that had been maintained by the domestic industry.

Mr. **Packard**. I won't take any more time. Thank you.

Mr. **Walgren**. Thank you, Mr. Packard.

Mr. Volkmer.

Mr. **Volkmer**. Maybe the panel would be able to tell me the approximate—we have a total amount that is spent basically on research and development in behavioral and social sciences in this country. How much of that would be spent in the private sector as against what is spent in the Government? Does anybody have any idea?

Dr. Abt.

Dr. **Abt**. My estimate was that it was somewhere on the order of $2 billion to $10 billion in the private sector and it's roughly $1 billion in the Government sector. Our numbers on the Government expenditure are much more accurate. In the private sector, it's a matter of definition. But it is certainly in excess of the amounts spent by the Government.

Mr. **Volkmer**. Do you or your organization and, Dr. Bray, in your organization, do you continue to do any type of basic research? Are you just a firm that is working basically with private industry and business in working with them with the behavorial knowledge that you have presently and trying to improve what they are trying to do in business and industry?

Dr. **Abt**. At Abt Associates, we just do applied research in the financial services, banking, automotive, pharmaceutical, and health care industries. We do no basic research.

Mr. **Volkmer**. Dr. Bray.

Dr. **Bray**. We do a little—we do, I would say, no basic research. I would say that my consulting firm does essentially no basic research, but very tentative basic research found elsewhere and serves as a middle ground between the researchers and the application of that research in industry, Government, and elsewhere.

Mr. **Volkmer**. What about Dr. Newhouse? You at Rand?

Dr. **Newhouse**. We do some basic research, largely applied research.

Mr. **Volkmer**. OK. What about Dr. Albers?

Dr. **Albers**. We do as much basic research as we need to allow us to develop the applied tools required in the particular discipline, but it's roughly 10 to 90 percent—10 percent basic and 90 percent applied.

Mr. **Volkmer**. Oriented toward a specific goal?

Dr. **Albers**. Yes, but it's still basic in the sense of the definitions of the discipline.

Mr. **Volkmer**. Where is all the basic research being done?

Dr. **Abt**. Mostly at universities.

Mr. **Volkmer**. In universities. Who would be funding that though? If the Government funds are down, universities don't have a lot of money.

Dr. **Albers**. More and more from the private sector.

Dr. **Abt**. Private.

Dr. **Coleman**. Not a great deal of basic research is funded in universities by the private sector. Very little of it.

Dr. **Bray**. Except that the private sector does employ many academics to do projects.

Mr. **Volkmer**. Basic research?

Dr. **Bray**. Often not basic research, but this does to some extent support the academic to do some more basic research back at home base related to the kind of work that person might be doing for the particular firm that employs him. Industrial psychology is a lot of interaction, interaction of that nature, but my feeling is that not enough basic research is done under the present arrangements.

Mr. **Volkmer**. Dr. Coleman, at the University of Chicago, how much basic research is being done in the behavioral and social sciences?

Dr. **Coleman**. We do as much as we can, primarily with funds from the National Science Foundation. But one of the problems that exists is that the peripatetic changes of mind that occur in the Federal Government affect more the funding for basic research and certainly affect it more in the funding for social science research, in social and behavioral sciences research in NSF and for applied or policy research.

Mr. **Volkmer**. Thank you very much.

Mr. **Walgren**. Thank you, Mr. Volkmer.

In pursuit of that, Dr. Coleman, as I understand it, the funding level for these particular areas of the social and behavioral sciences or at least a certain part of it, setting aside economics, setting aside perhaps psychology, has gone down in constant dollars some 50 percent since 1976. And I guess my question to you would be to ask you to comment on the adequacy of social science support in the National Science Foundation and whether that is a constructive course for us to pursue.

Dr. **Coleman**. No, I don't think it is a constructive course. I think that this was—this occurred as a consequence of the pique of a few people. Funding levels have gone up somewhat since 1981. So I think it is an unwise activity or unwise action on the part of the Federal Government if it expects to have a continuing supply of basic research in the social and behavioral sciences which can then provide the kind of background for policy research, for evaluation research, for natural and social experiments of the sort that Joe Newhouse described.

Mr. **Walgren**. Just a footnote, Dr. Coleman. You indicated the need for applied science schools as such, and at the same time asked for investment in capital and training to make social science more effective. There is a blurring of the basic and the applied in this whole area. It seems to be more critical than perhaps in some other sciences. We have no institutional responsibility for the applied side almost at all at this point, and that seems to be a pretty big gap.

Dr. **Coleman**. Let me say that the really serious problem lies in the fact that the disciplines, of the social sciences—and this is differentially true in different disciplines, but let me take my own, sociology—the disciplines are primarily oriented to training people to teach in undergraduate institutions. They are not primarily oriented to training people to do serious high quality research. And as a consequence, there are very few training programs in industry—in universities which are oriented toward producing the kinds of persons that can generate the kinds of research, the kinds of applied research that needs to be done.

Mr. **Walgren**. I have to stop at that point, and I apologize, Dr. Newhouse. By my count, I had 4 minutes to get over there, and it is about that far away, an appearance I hope you come to appreciate, Dr. Abt, in any future incarnations you might have.

But let me thank you on behalf of our whole effort here and express my appreciation and that of the others for your participating in this process. We may like to pursue you in writing and pull back some other suggestions in followup; and in that context, if there is anything you would like to send our way, we would be most appreciative in having it and, being a fully developed forum, we accept notes and handwritten thoughts as well as any. So with that, let me thank you again for coming, and we hope we see you again sometime.

[Whereupon, at 12:07 p.m., the task force adjourned to reconvene on Thursday, September 19, 1985, at 10 a.m.]

[Questions and answers for the record follow:]

Task Force on Science Policy Hearing on
The Role of the Social and Behavioral Sciences
September 18, 1985

Questions and Answers - Dr. Joseph P. Newhouse

1. Certain criteria may apply generally to the Federal Government's support of science. In making decisions about the support of the Social and Behavioral Sciences by the Federal Government, what criteria should apply?

For disciplinary-related research, I believe the criteria should be no different than the physical sciences. However, NSF might consider whether an ongoing review group that meets face-to-face such as the NIH study sections could improve the application of those criteria and hence lead to more efficient use of funds. Indeed, an experiment might be conducted. Both the usual mail review and a trial study section might consider the same proposals. If they reached the same conclusions, the cheaper mail review process would seem better. If they did not, further investigation would be warranted as to whether the face-to-face group obtained "better" results.

There is also a problem in supporting good empirical work (as opposed to theoretical) that is intended to illuminate policy choices. These projects tend to be large and long term. As I said in my testimony, there is a bias against such projects and support of such projects at appropriate levels may require a set aside--or at least report language to encourage such projects.

2. It is widely recognized that many of the changes now occurring in society are attributable to the advances taking place in the physical and life sciences. To what extent do the social sciences help the nation make informed use of the discoveries and technologies produced by the physical and life sciences?

The discoveries of the physical and life sciences often have unintended side effects such as toxic waste and pollution. Economics provides a framework for both thinking about tradeoffs involved and--up to a point--quantifying them. Basic research in this area should be supported by NSF; some applications work should be supported by both NSF and the relevant agencies, e.g. EPA. Other social science disciplines besides economics also probably have a role to play, but I have no special knowledge of their potential contributions.

Additionally, in national defense and health government is a major procurer of services. In these areas there is scope for publicly supported "technology assessment" (e.g. determine what medical services--and for whom--Medicare will pay for). This work should draw on methods developed in the social sciences. The work, however, is better supported from DoD and DHHS rather than NSF.

3. The deeper understanding of human needs and social processes which is expected to be achieved from research in the social sciences should provide an enhanced level of understanding relevant to national policymaking and priority setting in many areas. What contributions to national priority setting should the social sciences be making that they are not making now?

In general, the social sciences should be illuminating the tradeoffs that the nation faces. For example, in my testimony I cited the Rand Health Insurance Experiment. This project showed that for most employed individuals cost sharing could be increased with little or no reduction in health outcomes. Publication of these results has led to widespread increases in deductible and coinsurance by major employers. Poor individuals with high blood pressure or myopia were, however, worse off from cost sharing and their risk of death was correspondingly higher. Programs targeted at the poor sick thus may have a high payoff. Analogous work should be undertaken to determine what, if any, the adverse health outcomes are from freezing Medicare reimbursement to hospitals. NSF can contribute to the process of illuminating and quantifying tradeoffs--for example, through the Panel Survey on Income Dynamics that I cited in my testimony--but much of the contribution to inform policy choices must be made by research budgets in the agencies--and those are now seriously threatened.

4. Research in the social sciences has advanced steadily in the decades since 1945, and many findings have emerged. The use or nonuse of these findings is not, however, automatic, and may or may not be justified in particular circumstances. What is the rule of the Government in facilitating or inhibiting the contributions of the social sciences to the resolution of issues of national importance?

Government has a limited role to play in facilitating the use of social science research findings. Freedom of dissemination should be a general rule. This is not an issue at NSF, but can be an issue for contract research supported by the various departments. In general, the public interest will be better served the freer the dissemination policies.

5. In making judgments about the budgetary levels of support for the social sciences, the Congress has studied the proposed budgets for the individual disciplines, such as anthropology, economics, and psychology. But it has customarily authorized and appropriated funding in the aggregate, encompassing all of these disciplines in one lump sum. The diversity of the individual social science disciplines and their differing degrees of maturity and relevance to societal problem solving might suggest that the Congress differentiate between the levels of support to be provided. On the other hand, the ability to make such judgments, and the time required to delve into the state and opportunities in each field, might mitigate against such an approach. To what extent should Congressional support distinguish among the individual disciplines within the field of social and behavioral sciences?

Clearly someone must make a judgment about the benefits to be derived from different allocations of support among disciplines. The question is the degree to which the Congress should become an active participant in this process. I suggest that the Congress--and in particular the Committee on Science and Technology--provide an oversight function in this regard. NSF should be asked to justify its allocations among the disciplines, but the presumption would be that these were generally appropriate. This assumes that NSF has more information than the Congress on this issue, but that all fields are not equal and that the Congress should assure itself that it agrees with the rationale for NSF's choices.

Development Dimensions International

21 Knoll Road
Tenafly, NJ 07670
Feb. 11, 1986

Don Fuqua, Chairman
Committee on Science and Technology
U.S. House of Representatives
Washington, D.C. 20515

Dear Congressman Fuqua:

Thank you for your kind letter of Feb. 3, 1986 and for the opportunity to repond to additional questions of importance to the Science Policy Task Force. Unfortunately, I find the bulk of these questions to be beyond my expertise and so have little to add to my testimony. That testimony emphasized that basic research findings in psychology have been widely applied in business and governmental organizations to the benefit of those organizations, their employees, and the public. I urged the Federal Government's support to promote the basic research on which such applications can be based.

I believe these observations are relevant to your first question, the criteria for Federal support. Such support should go to ambitious basic research, not quick fact-finding to answer some immediate question. In supporting such research attention should be given to the likelihood that it will lead to application.

My only other comment is in respect to question 5. I do not believe that lump sum funding combining several disciplines is desirable. Psychology is, for example, very different from economics. Someone is eventually going to have to choose between disciplines in selecting projects. I feel that Congress should do that at the outset.

Yours very truly,

Douglas W. Bray

Douglas W. Bray, Ph.D.

RECEIVED
FEB 18 1986
COMMITTEE ON SCIENCE
AND TECHNOLOGY

Clark C. Abt

RECEIVED

FEB 18 1986

COMMITTEE ON SCIENCE
AND TECHNOLOGY

February 11, 1986

Chairman Don Fuqua
Committee on Science and Technology
U.S. House of Representatives
Suite 2321
Rayburn House Office Building
Washington, D.C. 20515

Dear Chairman Fuqua:

Attached please find my responses to questions raised by the House of Representatives Committee on Science and Technology concerning my testimony. I hope you find them helpful.

I would also like to invite you to visit Abt Associates Inc. in Cambridge whenever you can next be in the Boston area, so that I may have you meet our leading social scientists and research executives. I would also be delighted to see you in Washington on my next visit and will give you a call to see if that may be arranged.

Sincerely,

Clark Abt

Clark Abt

1. It is widely recognized that many of the changes now occurring in society are attributable to the advances taking place in the physical and life sciences. To what extent do the social sciences help the nation make informed use of the discoveries and technologies produced by the physical and life sciences?

Since the discoveries and technologies produced by the physical and life sciences find their principal applications in the support of human activities and behaviors, the fullest and most efficient application of any new physical science technologies inevitably involves social and behavioral changes. These social and behavioral changes are best observed, measured, forecasted, managed, and evaluated by the social sciences. Some examples: The main obstacle to fuller utilization of computing technology in offices and homes is the "user friendliness" of computer software, and this is a problem for social and behavioral research (particularly, psychology, economics, and industrial sociology). Another example, the impact of satellite communications and global television broadcasting on child and adult education, adult attitudes and political and economic behaviors and international trade and political relations can only be measured, managed, and evaluated with the use of social science surveys, predictive models, and statistical evaluations. Perhaps more urgently, the containment of the level and violence of international conflict instrumented by new weapons technologies and terrorist drives requires accelerated efforts in the social sciences to understand, predict, and control the most dangerous potentials of the new weapons technologies, particularly in their command and control communications, strategic planning for deterrence (itself a purely psychological process on which most of our lives depend daily), and the economic tradeoffs of balancing investments in economic growth, socio-political unity, and defense against aggression.

2. An important criterion for support of research in the social sciences must necessarily be the expectation that the resulting findings will contribute to the formulation of social policies, the solution of social problems, or the decision-making process at the judicial or regulatory level. To what extent has past social and behavioral science research in any of the disciplines contributed to the formulation of social and other policies, and what are the prospects for the future?

Economic research has greatly contributed to the establishment of national income accounts and the creation of social security, unemployment insurance and the one-third of the trillion-dollar federal budget spent on various social support programs. Psychological and sociological research has revolutionized American public education (and private, too) through the development of statistically standardized national testing. Further, psychological testing and training research has achieved enormous gains in the economy and safety of aircraft pilot training for both civilian and military purposes. This by itself is an example of how psychology has contributed to the saving of probably millions of lives in the last 50 years of civil and military aviation.

The prospects for the future are at their peak of brightness. It is only in the last two decades that we have been able to achieve truly interdisciplinary applications of the quantitative social sciences to the safest and most cost-effective implementation of physical and life sciences discoveries. Only consider the vistas of bio-medical advances ahead of it which must necessarily be a fundamental merging of the physical and social sciences.

3. The deeper understanding of human needs and social processes which is expected to be achieved from research in the social sciences should provide an enhanced level of understanding relevant to national policymaking and priority setting in many areas. What contributions to national priority setting should the social sciences be making that they are not making now?

The underutilized contribution to national priority setting that the social sciences should be making much more than they are now is in the design, operation, evaluation, and policy application of large-scale social and economic experiments. To paraphrase Socrates, the unevaluated program is not worth having. It is the social and behavioral sciences (which certainly include economics and statistics) that can tell us what works in government, what doesn't work, and how to find out if we don't know. There is no better way to improve the productivity of government and industry and get more benefits to tax-payers at reduced costs.

4. Research in the social sciences has advanced steadily in the decades since 1945, and many findings have emerged. The use or nonuse of these findings is not, however, automatic, and may or may not be justified in particular circumstances. What is the role of the government in facilitating or inhibiting the contributions of the social sciences to the resolution of issues of national importance?

The proper role of the government is to assure free access to all information and knowledge created by publicly financed social science research and development, unless there is a risk to national security requiring secrecy. It might surprise the American public to learn that such free access is not the case in many other free nations, not only in Communist countries. For example, the British Official Secrets Act makes it a punishable crime to divulge the results of any government sponsored research, however unrelated to military security matters, without specific authorized release by the political administration in power. One can imagine what inhibiting and distorting effects such bureaucratically controlled restrictions would have if we failed to persist in our American devotion to freedom of information. It is probably no accident that the funding for and the productivity of American social sciences R & D is much greater than that of the United Kingdom.

The role of the government in facilitating contributions of the social sciences resolution to important national issues is of course also to be the financier and market mandator of last resort, where public needs for social and behavioral knowledge are of a scale or of such a risky and controversial nature that private investments cannot suffice to get the needed work done with competent coverage and quality. Some examples; national educational testing, national research on the causes of criminal behavior, national research on the causes of both poverty and the continual emergence from poverty of entrepreneurs and others, the causes and consequences of successful and failed family formation, the causes of economically successful organizations of agriculture and industry in developing areas, the sources of the most productive industrial and economic behaviors.

5. In making judgements about the budgetary levels of support for the social sciences, the Congress has studied the proposed budgets for the individual disciplines, such as anthropology, economics, and psychology. But it has customarily authorized and appropriated funding in the aggregate, encompassing all of these disciplines in one lump sum. The diversity of the individual social science disciplines and their differing degrees of maturity and relevance to societal problem-solving might suggest that the Congress differentiate between the levels of support to be provided. On the other hand, the ability to make such judgments, and the time required to delve into the state of and opportunities in each field, might mitigate against such an approach. To what extent should Congressional support distinguish among the individual disciplines within the field of social and behavioral sciences?

Because scientific exploration and the creation of new knowledge by basic research is fundamentally unpredictable, a basic level of support should be provided to all disciplines that meet sound standards of intellecutal productivity conventionally applied by our institutions. To the extent that the applied research in individual disciplines is more or less relevant to current or national problems, a fair argument can be made that national resources should be allocated in proportion to the national priority attributed to the issues and the current productivity of applied research on that issue in that field. For example, basic research allocations should probably be made in proportion to the number of first-rate scientists in the field. However, for the priority problem of American industrial productivity, the disciplines of economics, industrial psychology and industrial sociology and management sciences (operations research) clearly have more to offer than many other disciplines one could think of, and should therefore receive more support than those other disciplines.

THE ROLE OF THE BEHAVIORAL AND SOCIAL SCIENCES

THURSDAY, SEPTEMBER 19, 1985

House of Representatives,
Committee on Science and Technology,
Task Force on Science Policy,
Washington, DC.

The task force met, pursuant to notice, at 10:05 a.m., in room 2318, Rayburn House Office Building, Hon. Doug Walgren (acting chairman of the task force) presiding.

Mr. Walgren. Welcome to the third day of these hearings on the social and behavioral sciences and their role in science policy. In the first hearing we tried to focus on the new areas of research that have been identified by the National Academy of Sciences' study that will be completed in the near future. Yesterday we tried to focus on some of the applications of the social and behavioral sciences by Government and industry and what the potential of those fields to improve productivity in the public and private sectors may be. Today we have a group of witnesses who will talk about things like the role of private foundations in the support of social and behavioral research, the role of social and behavioral sciences in policymaking, and some new developments in the area of human factors engineering which can have a lot of effect.

We want to welcome Dr. Francis Sutton of the Ford Foundation, Dr. Albert Rees of the Sloan Foundation, Dr. Amitai Etzioni of George Washington University, and Dr. Richard Pew of the Bolt Beranek & Newman Laboratories. We appreciate your coming and talking with us about this particular area. As you may know, in this Task Force on Science Policy, we have not focused directly on individual sciences, and this set of hearings on the social and behavioral sciences is a little unique in this pursuit because it does try to address a specific area, but it's also true that these are not specific social sciences and behavioral sciences. There are a whole host of subdisciplines within them, and in that sense even in this area we have not focused on individual sciences.

It's also true that in this general area the Government's involvement with the social and behavioral sciences has been more controversial and the support has been less forthcoming than in other sciences, and so we really did feel that this was a general area that the task force should focus on in hopes of developing the best recommendations we can that would make a constructive contribution to the future support and interaction between these areas and the Federal Government.

We do appreciate your making yourselves available to this process. In our process, written statements will be incorporated in the record that we make automatically, and you can certainly feel free to present as much from direct text as you would like or diverge from any direct text and emphasize points or develop points that you feel deserve to be underscored.

Let's go forward in the order that we called, and we will have all the testimony and then perhaps have a chance to discuss some of the subjects that you raise generally among ourselves and with other Members who might be able to come.

Dr. Sutton, why don't you lead off? Welcome to the committee.

[A biographical sketch of Dr. Sutton follows:]

Dr Francis X. Sutton

Born: 1917.

Married, 4 children.

Education: B.S. Temple, 1938; M.A. Princeton, 1940 (Mathematics); M.A., Ph. D. Harvard, 1941, 1950 (Sociology).

Flying Officer, U.S Army Air Corps, 1941-45; Junior Fellow, Society of Fellows, Harvard, 1946-49; Assistant Professor of Sociology and General Education, 1949-53; Lecturer, 1953-54.

Ford Foundation: Executive Associate, Behavioral Sciences Program, 1954-57 Program Associate, Middle East/Africa Program, 1957-63; Representative, East and Central Africa, 1963-67; Assistant to the Vice President, and Head, Planning and Evaluation Staff, International Division, 1967-68; Deputy Vice President, International Division, 1968-83; Officer in Charge, European and International Affairs Program, 1967-68, 1970-71, 1977-80; Acting Vice President, International Division, 1981 Consultant, 1983 to present.

Served at various times as visiting professor and lecturer at MIT, UCLA, etc.; member, Board of Foreign Scholarships (U.S. Department of State), Commission of International Education (American Council of Education); consultant, Harvard Institute of International Development and Center for Population Studies. Currently member: Plans and Policy Committee (Social Science Research Council) and International Advisory Committee (SSRC and ACLS); Council on Foreign Relations; African Studies Association; Century Association. Distinguished service award, Association for Asian Studies, 1984.

Acting President, Social Science Research Council, October 1985 to present.

Author of *The American Business Creed,* and many articles and addresses on social scientific, philanthropic, educational and developmental subjects.

STATEMENT OF DR. FRANCIS X. SUTTON, CONSULTANT, FORD FOUNDATION, NEW YORK, NY

Dr. Sutton. I am Frank Sutton. I have been for 30 years a Ford Foundation official. Currently I am a rather superannuated official. I am retired, and I am a consultant at the Foundation, doing a history of the Ford Foundation's international activities. If you notice a certain historical trend to my prepared statement, it is a reflection perhaps of my current preoccupations.

I began my education in mathematics and drifted over to sociology. I taught at Harvard for some years and then went to the Ford Foundation for a brief stay which turned into 30 years. I spent quite a number of years or some years there with the behavioral science program, which is I guess one of the historical modes of foundation support of the social and behavioral sciences. After that I spent most of my time on international matters which involve a great deal of social science, and I want to say a little about that in my testimony.

I am currently, in fact, spending about half my time working for the Social Science Research Council, and I will become acting president of that on October 1, but the Social Science Research Council doesn't have direct responsibility for what I am saying here, nor does the Ford Foundation. I have no current responsibilities for the Ford Foundation's programs, and I cannot speak with authority on the Ford Foundation—only experience. I am therefore speaking here as an individual.

Mr. WALGREN. Fine.

Dr. SUTTON. I want, I think, to first comment something about what I regard as the remarkable importance of the social sciences in the American contribution to the development of the sciences. It seems to me that the unique American role in the development of the sciences has in fact been in the social sciences and not in the natural sciences, however splendid the present accomplishments and distinction of these are. Indeed, the humanities we borrowed from Europe in their practice, and we modeled ourselves as continuators and extenders of European practice, but not in the social sciences. It seems to me that there is a peculiar historical development in the United States where we had, for the first time in the world, a really extended, serious, far-flung professional devotion to the social sciences here in the United States. When this history comes to be written with, I trust, due judgment and long enough perspective, I think that will stand out.

I want at the same time to emphasize a rather paradoxical fact that it has been rather hard to support the social sciences as such. We have made constant use of the social sciences and, as I shall argue, we make more and more use of them, inevitably, inescapably, but it has not been easy to support them. The basic reasons I dwell on in my written statement here with some examples. I think they have to do basically with the ambivalences that we have over the relationships of practical knowledge and experience with the more systematic knowledge that is acquired and developed by specialists in various fields. We have this in all fields, but it is particularly troublesome in the social sciences, where we all think we are something of experts and where we know that there are many matters that are not to be handled by the judgment of experts but require the experience of people who have been at the matter.

That balancing is very difficult and has upset all sorts of things at various times. The difficulties which I illustrate in some of what I have said in this prepared statement, of explaining to lay boards what social scientists are doing and why they want to study some of the things they do and why they speak about them. I have had much personal experience of this, and it is not an easy undertaking, as I am sure all sorts of evidences here in Washington show.

The upshot of all this has been, I think, that in the foundation world there has been a great deal of support of the social sciences. As a matter of fact, I was astonished when at a late point before coming here I was given a copy of this study that the Congressional Research Service has prepared. The magnitude of the numbers there rather startled me. They are very hard numbers to accumulate and the definitions and the boundaries are very uncertain, so I don't quite know how to assess them myself. I was indeed astonished at the level of social science support from the Ford Foundation. I would not have thought it was that high currently, but it is plausible because there are so many subjects in a philanthropy that tries to be rational philanthropy, and to get at the subjects in some generality and with some authority, it is inevitable that one is constantly supporting studies of various questions that seem not to be well understood or that have not been brought into some orderly focus.

All this does not mean that it has been easy to support the social sciences as such. The history of this is a very tangled matter. We are slowly now coming to understand it, I think, a bit. There were efforts early in the century to develop the direct support of the social sciences by the Carnegie and Rockefeller philanthropies, which fell on ill days before very long. The most substantial development was that of the Laura Spelman Rockefeller fund in the 1920's, and I think that owed something to the peculiar genius of Beardsley Ruml, who led that organization through the critical years from 1923 to 1929.

There have been other efforts, and I am sure my colleague here, Albert Rees from the Sloan Foundation, will speak of others. I want to jump ahead to say something about the fact that the Ford Foundation began in 1950 with a strong urging from the study committee that laid down its program, that it advance the understanding in a scientific way of human behavior. Hence the behavioral science program at the Ford Foundation, which lasted only until 1957 and had a rather troubled career. The first president of the Foundation didn't believe in the subject, nor did his chief ally. Many of the trustees had difficulty dealing with the subject. There were others who were for it. It illustrates some of the difficulties that I mentioned before in this ambivalence over the extent to which you can get ahead with the practical business of the world by understanding it more systematically through the social sciences.

Since the demise of that behavioral science program in the Ford Foundation in 1957, I was fired at that time from the Ford Foundation as a member of the staff of that organization, but shifted then to the international side. We eliminated the program, and it has not been revived, and there is to my knowledge no comparable program that has been revived, either by the Ford Foundation or others, for the general support of the social sciences as such—not their applications, not a set of social problems, but to the advancement of the sciences as such.

This is not to say, as I said earlier, that there is not a very great lot of support of the social sciences. I always stumble over whether I want to talk specifically about the social sciences or the social and behavioral sciences. The boundaries are, of course, very obscure in all sorts of respects. I mentioned in the prepared statement that the Ford Foundation took the label "behavioral sciences" because we wanted to be sure to include psychology, which some people don't want to call a social science, or at least in some parts thereof. Also, "behavioral" sounds a little more scientific than "social" science—"social" which gets mixed up with some other closely related sounding words. But I don't want to suggest by referring in shorthand to the social sciences that I have some kind of narrow prejudice against the behavioral sciences. I don't want to say always "social and behavioral," so I hope you will understand it's an ellipsis.

There is a great deal of support of the social sciences, but it has declined over the years because the general level of foundation support of all sorts of activities has declined. I put a table in the prepared statement which is the kind of table I never put together before for the Ford Foundation, and I have never seen it. It is on page 12 of the prepared testimony, and it shows something about the decline in the total Ford Foundation expenditures over in the last column, as well as in international studies, which was the particular focus of attention at that point in the paper.

FORD FOUNDATION COMMITMENTS AND EXPENDITURES

	International studies (in thousands)	International relations and world problems (in thousands)	Total (in thousands)	Ford Foundation expenditures (in millions)
1965	46,593	4,724	51,317	299.5
1966	47,190	4,457	51,647	362.2
1967	25,954	3,438	29,392	262.6
1968	6,659	4,236	10,805	210.2
1969	6,617	4,977	11,594	238.0
1970	13,170	5,206	18,376	236.8
1971	9,372	4,191	13,563	225.1
1972	11,550	2,935	14,485	218.5
1973	8,865	6,315	15,180	237.0
1974	9,643	7,351	26,994	251.6
1975	7,394	4,441	11,835	204.6
1976	5,852	7,965	13,866	159.4
1977	2,561	8,014	10,575	124.0
1978	4,604	7,758	12,362	129.9
1979	2,222	10,172	13,394	107.8
1980	3,723	7,984	11,707	114.1
1981	2,541	9,860	12,401	122.4
1982			12,268	135.8
1983			10,406	127.9
1984			24,320	167.0

Note.—Total expenditures as here summarized include program activities, program and general management costs, but not the costs of raising income or the Federal excise tax. Such was the practice in public reporting of expenditures to 1980; subsequent to that year the reporting practice changed but the figures have been adjusted accordingly. Since the reorganization of programs after 1981, international studies have been included in the totals for international relations.

If you look at the numbers in the last column, the Ford Foundation was in the midsixties committing $300 million a year or more. These numbers include administrative and program costs as well as direct grants. That's the way they are reported. It could be separated out, and I could discuss that but I won't pause to do so. If you notice, the decline is very sharp until the bottom is reached for the Ford Foundation in 1979 at just a little over $100 million. You must remember that these were the periods of great inflation, so that to understand the weight of what was done in the midsixties in comparison with what is done in recent years, you have to introduce a factor of 3, perhaps, so that you have in effect a reduction in the real contribution of these moneys of perhaps 5 to 1, so that there is a very substantial decline here.

This is not really peculiar to the Ford Foundation. The bad capital markets and the inflation of the seventies were very damaging to foundations in general, and I think there has been a loss of relative weight in the total panoply of American institutions that is not likely soon to be recovered. This is, I think, a very important point as between the distribution of what can be done by foundations versus what can be done by public agencies, the Federal Government, and others.

Now as a part of this shift there was a notable decline in the Ford Foundation support of general university development, with major research universities in particular, graduate fellowships of general kinds, and these were very large programs for the Ford Foundation. That was the era when everyone believed in university development and believed that foundations were big enough do something about general university development. Hence, very large sums went into the universities and into the support of graduate study. I give some details in the prepared statement on this.

How much of that went to the social sciences is an exercise that no one has ever done for the Ford Foundation, but it was obviously very substantial. That now is mostly gone. There are special fellowships for minority students, disadvantaged, and so forth in the Ford Foundation, but there is not the general kind of support that there once was that gave a great deal of support to the basic social sciences in the universities, so that there has been a removal of that sort of thing.

I dwell a little, also, on international matters because there was a great development of international studies in the 1960's particularly, the 1950's and 1960's, that has given this country an unparalleled capacity I think in the universities and elsewhere to understand the complicated international world in which we live. That was very heavily supported by the foundations.

You see in this table that I have prepared on the extreme left, the column, the very sharp decline in what Ford Foundation did in that. Ford Foundation put something like $270 million into the universities through 1967 and thereafter fell off very sharply. There is a danger, it seems to me, that unless others have carried on in that way, that there will be a threat to the maintenance of an extraordinary panoply of institutions which seem to me important to the United States in a very general way, and certainly important intellectually.

One of the things that has attracted me to the Social Science Research Council now is that it is an important means of linking American scholarship to scholarship around the world at a time when this is very much needed for both intellectual and political and cultural reasons, and also because the social sciences are intrinsically international undertakings. They are general subjects, and they ought to be pursued in international cooperation. Some of that I think is threatened by the relative decline in support of international studies from the foundation world. Hence, the continued importance of Federal support in this particular matter, as in so many other matters related to the basic social sciences.

I do not see that there is likely to be any early fundamental change in the support of these subjects in the foundation world. There has recently been a most welcome and abundant increase in attention to what we always called in the Ford Foundation "international affairs"—studies of international security, strategic studies, international economic problems, worrying about problems of migration and refugees, that array of subject matter. That has risen very encouragingly, as you can see in some of numbers in the table I have given, and it is paralleled in efforts of other foundations. All of this seems to me to be a most encouraging and needed kind of effort.

It does not, however, reach very far into the problems of the basic social sciences, and does not I think fundamentally affect the view that I would put before you, that while the foundations are strong in the support of the social sciences, they do not do much in support of the basic social sciences as such. There are exceptions, and I trust this will come out in the panel.

Is there any prospect of any significant change in some of this? Much of this depends upon the attitudes that people have toward the social sciences, the kind of thing that you people here are, I'm sure, very much aware of. I think that we have been in a period in which the social sciences have been subject to skepticism and criticism much greater than in the past. It is not peculiar to the social sciences. We are in an area in which criticism and skepticism is greater than it was in the past, and I think this is not necessarily bad. As one becomes more sophisticated, as the population of this country becomes more educated, more people raise their voices, there is more contention. There are many difficulties in this. I think that it is only natural that there have been these contentions.

I think there has been a special influence in that the kind of cultural change that came over this country in the late sixties and the early seventies brought a kind of—a reaching for direct experience, a kind of skepticism about rational analyses and general theories. I like to think of it as being like the romantic revolt after the enlightenment. Now this sort of thing is still with us and will be with us, I think, for a while, but such tides fall after they have risen.

One sees, I think, both gains from this change in many of the things that have gone on in the social sciences, and one sees also prospects of a different set of attitudes. I am myself hopeful that some of the changes that have been apparent in attitudes and in the views of the professionals in these fields themselves have been beneficent and will continue to be valuable—the rapprochement between the sciences and the humanities that has gone on seems to me to have been related to some of the skepticism that I was speaking of. It has also been related to a certain correction of a kind of positivistic overconfidence that we had in the capacity of social sciences to achieve things, so that there is a prospect, I think, of some better days ahead both in the matters of practical performance and in the attitudes that relate to the social sciences.

I have put in several examples here that I am tempted to cite to you as to the inescapability of social sciences, one particular one that I was very struck by, coming to Washington. These figures that I have put in on page 17 about the extraordinary increase in the fractions of the Federal budget which are committed through statistical formulae and the very sharp rise in the percentage of budget expenditures—these are figures for 1971-81—57 percent, according to this study, were linked directly or indirectly to the CPI or other indices, whereas in 1966 only 2 percent were automatically indexed.

This is a striking example of the extent to which we have become more and more dependent upon statistics and hence on the theoretical research on which they are based. It is inescapable. We will grumble, but we will go ahead in this fashion, inescapably, in the kind of society we live in, so that I don't see any danger of neglect to these social sciences ahead in the future, and I think that there may be a return to a more positive general set of attitudes.

I don't want to hold out the prospect that there would be a great upsurge of general assistance in the development of the basic social sciences by the foundations. The reasons are partly in the diminishment of their relative weight. There is no escaping the fundamental role of Government and public agencies in the support of developments in the social sciences, as in other sciences now, but I do think that there will be plenty of attractions and opportunities for the private foundations, and I look forward to their doing more.

Thank you.

[The prepared statement of Dr. Sutton follows:]

TESTIMONY OF FRANCIS X. SUTTON, FOR SCIENCE POLICY TASK FORCE, HOUSE COMMITTEE ON SCIENCE AND TECHNOLOGY

September 19, 1985

I am Francis X. Sutton, currently a consultant to the Ford Foundation and after October 1, to become Acting President of the Social Science Research Council in New York. I have spent more than thirty years with the Ford Foundation, from 1954 to the present time. My education was in mathematics and sociology at Temple, Princeton and Harvard. At Harvard I took a Ph.D. in sociology and taught there until coming to the Ford Foundation in 1954 as executive associate in the behavioral science program. In 1957 when the behavioral science program was terminated I shifted to the overseas development program, working principally on and in Africa for the next ten years. In 1967 I returned from Kenya where I had been the Foundation's representative for East and Central Africa, to the New York headquarters and shortly thereafter became Deputy Vice President for the international division, a position which I held until my retirement in 1983. Since my retirement I have been engaged as a consultant on the history of the Foundation's international program and have been engaged in various other activities, among them membership in various Social Science Research Council committees and service as chief consultant at Harvard on the planning of the Aga Khan University. Throughout my career I have written fairly extensively on topics in the social sciences, education, development and philanthropy. I should emphasize that while I am still a consultant to the Ford Foundation, I have no responsibility for current Foundation programs or policies, and must testify here as an individual, and in no sense as an authorized representative of the Ford Foundation.

The support and use of the social sciences by American foundations has been so extensive and pervasive that it is nearly impossible to give a comprehensive view of where it begins and ends. Modern philanthropy has sought to do more than comfort the afflicted. It has aspired to be rational philanthropy, getting at the roots of human ills and deprivations. Judgement, experience and common sense are as much needed in foundations as elsewhere, but they need to be bolstered by the kind of specialized knowledge and orderly investigation that the social sciences supply. The bigger foundations that aspire to deal with national and international problems feel these needs particularly acutely. Whether they are trying to improve health services, find better models of day care centers to help working women, or trying to improve thinking about international economic policy, they need some orderly analysis of the subjects to proceed in the confidence that they are doing something sensible. And characteristically, in dealing with any broad subject there comes to be a concern with the intellectual and professional resources in this country or in the world for dealing with them.

Foundations thus use the social sciences a great deal whether they are consciously supporting them or not. But at the present time there are very few foundations with the declared purpose of supporting the social sciences--too few, in my personal judgement. There is, indeed, much less declared concern with the state of the social sciences than there has been in the past. While there is a continuing effort to deal rationally with a broad array of social and human problems this does not lead to substantial concern with the state of the social sciences as such. Marshall Robinson of the Russell Sage Foundation offered some estimates in 1983 indicating that foundation funding for social science research had not declined significantly in absolute terms and had indeed increased enough as a percentage of total grantmaking to compensate for the overall decline in big foundation spending that went through the 1970s.1) Such estimates are subject to grave uncertainties, but my own judgement is that the broad conclusion is sound--there has been no foundation retreat from the use of the social sciences in the subjects of their interest but a marked decline in concern with and support of the social sciences as professional and academic enterprises.

The Decline of Foundation Support for the Social Sciences as Such

There is a serious historical argument to be made that the most distinctive contribution of the United States to the development of the sciences has lain in the social sciences. However distinguished American

---

1) Robinson calculated that in 1964 "the big four in social science"--Carnegie, Ford, Rockefeller and Sloan--made grants totalling $320 million of which about 11% went for social science research; in 1980 these four granted 17% of a total of $160 million for social science research. See SSRC Items, volume 37, nos. 2/3, September 1983, p. 37.

physical and biological sciences have become in this century, they have been offsprings and continuators of European pioneering. Our humanistic scholarship likewise had its roots in Europe as well. The social sciences as far-flung academic and professional enterprises seem to have been more natively American. While philanthropy can claim some credit for this American flowering, it owes more to other influences--among them certain distinctive features of American higher education as it developed across the country, a peculiarly American disposition to think that large parts of human affairs were manageable by knowledge and techniques accessible to anybody regardless of their ultimate beliefs and affiliations, political or otherwise; and finally to our optimism about the perfectibility of human affairs through enlightened effort. Despite this natural American disposition toward the social sciences, they have not been easy to support, either through government or foundations. In the old days when Americans kept the federal government limited and foundations like Andrew Carnegie's and John D. Rockefeller's had great opportunities to fill in where public monies were lacking, they found education, libraries, the health and natural sciences easier to support than the social sciences. The Carnegie Institution in Washington sponsored a department of economics and sociology in 1902 but later abandoned it; the Rockefeller Foundation briefly flirted with economics research under MacKenzie King but were scared off by the Walsh inquiry of 1915. It took the special genius of Beardsley Ruml using the Laura Spelman Rockefeller Memorial monies in the 1920s, to make a significant and lasting foundation contribution to the development of the social sciences. By the 1920s there was in the American universities a group of leaders who wanted more objective and empirical study of society than the social reformers practiced and the Spelman Fund gave lasting impetus to the social sciences in American universities, the Social Science Research Council, the Brookings Institution, and the London School of Economics.[2] By 1929 when the Spelman Fund was incorporated in the Rockefeller Foundation, it provided the base for a social science division which has continued to this day. It was not long, however, before the Rockefeller Foundation abandoned the block grants to universities for the social sciences that Ruml had favored. The social science division turned towards more focussed support of research, maintaining a concern with basic social science but gradually abandoning a general concern for the state of the social sciences.

In Rockefeller's tradition a strong concern for the sciences has favored more direct attention to academic disciplines and basic research than Carnegie and Ford have had. Both these latter have been more disposed to begin with attention to a major national or international problem and to engage social scientists in it, often with important effects on the social sciences themselves but only incidentally to their major purpose. Hence, for example, Carnegie's support of the study of American race relations in Myrdal's An American Dilemma and its initiation of the Russian Research Center at Harvard. When Ford came on the scene, at the turn of the 1950s, it was

---

2) Martin Bulmer and Joan Bulmer, "Philanthropy and Social Science in the 1920s: Beardsley Ruml and the Laura Spelman Rockefeller Memorial, 1922-1929," Minerva 18, #1 (Winter 1980), pp. 347-407.

committed very broadly to the advancement of human welfare and the study group that set its program recommended that "improved scientific knowledge of individual behavior in human relations" was one principal way it should try to do so. A behavioral science program was duly established in 1951 that was looked to with hopeful excitement across the country--the label "behavioral science" being chosen because it could clearly embrace psychology and because it was thought to suggest a more firmly scientific approach than "social science" did. This program had a troubled career, however, and was terminated in 1957. Why this should have happened was a matter of some personal interest to me, and a little account on this bit of Foundation history may be helpful in understanding why foundations have not been more active in support of the basic social sciences.

There have always seemed to be ambivalences in American attitudes towards the social sciences and they were particularly marked at the time the Ford Foundation appeared on the scene with a new program in 1950. There was on the one side a surging optimism about America's strength and power. The contributions of science and technology to American strength were appreciated with new enthusiasm and some of this enthusiasm extended to the social sciences. The study committee report that laid out the new program for the Ford Foundation was only one among many examples of the time where faith was expressed in the necessary place of these subjects in a hopeful human future. But there were critics and suspicious doubters too, both within and outside the Foundation. The hearings on the National Science Foundation in the 1940s showed some of these; others appeared in the Cox and Reece investigations of foundations in 1952 and 1954. There was on the one hand the suspicion that the social and behavioral sciences could not be divorced from ideology or advocacy and as such made false or misleading claims to the status of objective sciences. Social scientists were in particular thought to have liberal or leftish biases and in the tremulous anxieties over national loyalty that marked the time, they were thought by many to be "socialistic" or worse. There was also the doubt that these were or could be successful sciences, that their discoveries were few while they pontificated on the obvious, or obscured the familiar in clouds of jargon. Views of these sorts had their representation within the Foundation. The first president, Paul Hoffman and his principal associate, Robert Maynard Hutchins, had little faith that the behavioral science program they were charged to develop would be fruitful. They thought the record discouraging and they were glad to turn over to Rowan Gaither, who had recommended the program in the first place, the responsibility for developing it. Gaither undertook to do so, first as an associate director and then as president of the Foundation. But the trustees were not happy with the program and it lasted only a few months after Gaither resigned the presidency in 1956 (to become chairman of the board). Basically, the demise of the program seems to have been due to the difficulties of making a program that had long-term academic and intellectual aims in these fields intelligible and credible to laymen. Ultimate lay control of any scientific or intellectually specialized endeavor always involves a delicate mediation through faith and authority, and the belated public support of scientific research in the United States suggests that it may have been particularly difficult in this country to organize ourselves for such endeavors. The very self-reliant individualism on which we have prided ourselves has made us reluctant to surrender to the authority of experts and such surrender has been particularly difficult in matters that touch our intimate lives or seem to lie within the realms of practical judgement and experience. It has often been argued that the paroxysms of concern over loyalty which marked the 1950s came from diffuse anxieties over changes that were diminishing Americans' confidence of being able to control their own existences with what they had learned at their fathers' and mothers' knees. I believe there is something in the argument. With the decline of traditional beliefs we have all become more dependent in our individual lives and our society for guidance from the "best, up-to-date knowledge". Such dependence on knowledge possessed by specialists provokes resistances even in matters such as health where we have had much more tolerance for unintelligible Latinities than we have for the jargon of social scientists. While there was more widespread confidence in the 1950s than later that an objective, value-free social or behavioral science was a real possibility, the atmosphere had turbulence and instabilities from the sources I have been describing. They had their effects on Ford's behavioral science program and it was also handicapped by certain organizational factors. One important such factor was the practice at that time of requiring direct trustee approval of most grants; it meant that social science research projects had to be presented directly to a lay board; this procedure gave maximal opportunities for doubts and questionings that did not arise in some other foundation programs which were in fact supporting a great deal of social science research but did not have to explain its substance to the trustees.

After the behavioral science program was abandoned in 1957, no comparable program was revived by the Ford Foundation nor has any other of the big foundations come along to replace it. The Ford Foundation in fact continued to be a major supporter of the social sciences, and, social science research in particular, for many years thereafter. It did so directly in economics throughout the 1960s. But the greater part of the support came indirectly, through massive support of universities and colleges, through general fellowship programs for graduate students, and through its international programs. How much of the $349 million the Ford Foundation provided in challenge grants to 84 colleges and universities between 1960 and 1970 went to building up the social sciences would be a difficult investigation that no one has ventured, but the amounts were certainly substantial. Likewise, some historian of the social sciences in this country may want to know how many of the Woodrow Wison fellowships went to budding social scientists or how much of the $41.5 million for graduate education given to 10 major universities between 1967 and 1972 went to social scientists. I recall these programs not to celebrate what the Ford Foundation has done but to underscore a very important change that has occurred since the 1960s. After that decade the broad support of major research universities that had been a part of Ford's programs fell away almost completely, and with this decline and fall a major source of foundation support for the social sciences passed away.

A similar decline came about following the termination of the International Training and Research Program in 1967. This program had been far and away the largest private source of support for the development of area studies and other types of international studies in the American universities, with commitments of about $270 million by 1967. The great bulk of these funds went to support training and research in the social sciences and humanities. There was a bias toward emphasizing the social sciences and the contemporary scene over the past, and it would certainly not be wrong to claim that more than half these funds went to the social sciences. Support for international studies continued through the Seventies, though at a lower level and with a rather substantial shift in character. It was the Ford Foundation's practice for many years to distinguish "international studies" from "international affairs", the former being mostly concerned with the academic study of world areas, the latter more with policy analysis and other approaches to dealing with U.S. foreign policy and world problems in security, international economics, and other fields. The following table shows the very marked shift in the balance between these types of support from 1965 through 1981. 1965 was almost the peak year for international studies--it was slightly overtopped in the next year, but decline was then sharp and rapid with occasional revivals through special programs or terminal endowments that were made to preserve something for the future. International affairs includes many activities that are academic in base and character and many that are not. It leans heavily on the social sciences but characteristically in their application to problems of security, international organization, regional questions, or economic problems. Whatever the relative merits of these types of activity may be for the service of the public good, it seems probable that international studies have been more closely and generally related to academic social science than the policy studies of international affairs have been. Thus while the Ford totals for these types of support show an interesting constancy through the Seventies and the beginning of the Eighties, the strong shift toward international affairs suggests a shift toward more applied or specialized social science.

FORD FOUNDATION COMMITMENTS AND EXPENDITURES

	INTERNATIONAL STUDIES (in thousands)	INTERNATIONAL RELATIONS & WORLD PROBLEMS (in thousands)	TOTAL (thou)	TOTAL FF EXPEND. (in millions)
1965	46,593	4,724	51,317	299.5
1966	47,190	4,457	51,647	362.2
1967	25,954	3,438	29,392	252.6
1968	6,569	4,236	10,805	210.2
1969	6,617	4,977	11,594	238.0
1970	13,170	5,206	18,376	236.8
1971	9,372	4,191	13,563	225.1
1972	11,550	2,935	14,485	218.5
1973	8,865	6,315	15,180	237.0
1974	9,643	7,351	26,994	251.6
1975	7,394	4,441	11,835	204.6
1976	5,852	7,965	13,866	159.4
1977	2,561	8,014	10,575	124.0
1978	4,604	7,756	12,362	129.9
1979	2,222	10,172	13,394	107.8
1980	3,723	7,984	11,707	114.1
1981	2,541	9.860	12,401	122.4
1982			12,268	135.8
1983			10,406	127.9
1984			24,320	167.0

Total expenditures as here summarized include program activities, program and general management costs, but not the costs of raising income or the Federal excise tax. Such was the practice in public reporting of expenditures to 1980; subsequent to that year the reporting practice changed but the figures have been adjusted accordingly. Since the reorganization of programs after 1981, international studies have been included in the totals for international relations.

Two general observations arise immediately from examination of this table. One is the very steep decline in the Ford Foundation's overall expenditures from the heights of the mid-Sixties to the low levels in the latter half of the Seventies, with some recovery in the Eighties. The decline would be much more dramatic in constant dollars and similar declines appeared in other foundations. Nearly all of the major foundations were damaged in their strength by the combination of inflation and adverse capital markets through the 1970s and early 1980s and despite the fact that overall foundation philanthropy has grown in nominal terms in recent years, reaching some $3.6 billion in 1983, one must observe with melancholy a decline of the relative weight of foundations in meeting American needs. The Ford Foundation did not reduce the fraction of its total commitments going to international matters. There was, however, the sharp decline in "international studies" which was only partially compensated by fresh commitments of other foundations such as Mellon and Hewlett to these subjects. The funds the Ford Foundation put into the development of area studies programs in American universities in the 1960s contributed, along with federal funds under the National Defense Education Act, to what the recent report of the Association of American Universities calls "the creation of institutions unmatched anywhere in the world, a national resource whose loss would immensely impoverish the capacity of our democratic society and our government to understand the complex, interrelated world in which we live."3) With reduced foundation funding, the

---

3) Richard D. Lambert et al., Beyond Growth, Association of American Universities, Washington, April 1984, p. 9.

indispensability of Federal funding for these institutions, now through Title VI of the Higher Education Act, is evident if these institutions are to be preserved. The reduced resources of Ford and and other foundations in real terms mean that these and other great investments of the 1960s could hardly be repeated even if there were a strong disposition to renew them. A major new thrust in private foundation support of area studies seems unlikely in the near future in any case. The disposition that the table shows for Ford to favor policy and other studies in international affairs is one that is paralleled in other foundations, as we see in the recent commitments of MacArthur, Carnegie, Rockefeller, and others in international peace and security studies. We all have welcomed this upsurge of interest in the awesome security problems of the nuclear age, and there are many fresh challenges to the social sciences they present, but we must remember that they draw on at least as much as they nurture the disciplinary resources themselves.

One further aspect of foundation interests in international matters remains to be noted. A great internationalization of the social sciences has occurred during recent decades, both in the range of American interests and in the growth of professional social science communities around the world. Assistance to the developing countries has required the growth of competences in the social sciences and this has been a particular interest of foundations. I am most familiar with what the Ford Foundation has done, but there have been major contributions by others, notably the Rockefeller Foundation through its Education for Development and other programs. The conception that indigenous competences in the social sciences are indispensable to developing countries has weathered many changes and disillusionments in development strategy. It is hardly conceivable that modern nations can cope with their national and international problems without professional economists and other social scientists, and it is one of the more striking features of Ford Foundation policy in the 1970s that the building of such competences persisted vigorously through the painful budget reductions that had to be made during that decade. The great training resources of the American universities have been indispensable to this effort and a large fraction of the world's social science leadership has now had American training. I believe these relationships have intellectual importance as well as the evident value they have in the international relations of the United States. They help keep our social scientists from parochialism and they stimulate attention to large bodies of subject matter that have been threatened with neglect as the U.S. has become more preoccupied with its own problems and fewer Americans have been engaged in technical assistance. As I was preparing my remarks for this occasion I happened to read a review by the great biological scientist, Max Perutz, of a book on the future of science and I was happy to find him criticizing the author for seeing "the future as a collection of technological fixes in the United States, but not [considering] how science might be used to eliminate poverty, ignorance and disease in the rest of the world. This surely is our greatest challenge."4) We have a tendency nowadays to neglect

---

4) Review of Gerald Feinberg, Solid Clues: Quantum Physics. Molecular Biology and the Future of Science, New York Review of Books, September 6, 1985, p. 17.

this challenge unless our attention is forced by African famine or fears of invasion by destitute masses. We must not do so, and if experience with aid programs has taught us anything it is that the problems of development are more difficult than we thought and not to be mastered without better research and analysis than we have thus far brought to them.

The social sciences, by their very nature, are and ought to be international enterprises, and it is very much in the interest of this country to sustain the resources which make us attractive to students and mature social scientists from abroad, and that assure our capacities to work with them. I am proud and happy that the Social Science Research Council has an important role in these relationships, which it plans to sustain as well as its resources permit. But I have some serious disquiets over the decline in recent years of resources from the foundations and elsewhere, that make possible graduate training of foreign social scientists here and that enable our own social scientists to travel and engage in cooperative research abroad.

I must return now from these international aspects of American social science, important as they are, to a broader concern with the state of the social sciences and their needs.

There has undoubtedly been a weakening of faith in the capacities of the social sciences to guide us reliably toward solutions of society's problems. The social sciences are not peculiar or special victims of contemporary skepticism. The questionings and mistrust that have come over American society since the 1960s have reduced faith in government and most of our institutions. The sciences generally have not escaped questioning of their beneficence, and sometimes have had to face downright hostility. While we have nostalgias for the more trusting and optimistic America of the past, we cannot deplore the growth of the more critical and discriminating assessments amid which we now live. I conceive that abundant contention is an inseparable consequence of that fundamental tendency of modern societies that the great German sociologist, Max Weber, called the process of rationalization. As this process goes on, our governing institutions come under more and more pressure to plan and justify actions as objectively and rationally as possible. I was impressed to read recently that $122 billion or about one-fifth of the total federal budget obligations in fiscal 1979 were committed through statistical formulas, and that some 57% of all budget expenditures in 1981 were indexed directly or indirectly to the CPI and other indices, whereas in 1966 only two percent of the budget was automatically indexed.5) Our reliance on statistics and the research on which they are based grows awesomely, and will not be reversed whatever the criticisms that abound over them. And in our private lives the magazines and newspaper sections and even television programs telling us the latest research "findings" on how we should run our businesses, our families and ourselves, shows a similar rising luxuriance. We complain about our social and behavioral guides as we grumble about doctors, but we eagerly submit.

In such circumstances one might suppose that there would be more anxious concern over the state of the social and behavioral sciences than there in

---

5) James Bonnen, "Federal Statistical Coordination Today: A Disaster or a Disgrace?", The American Statistician, August 1983, v. 37, no. 3, pp. 179-192, at page 184 with references to the relevant sources.

fact seems to be. I have at the outset briefly described the decline of foundation attention to the basic social and behavioral sciences. There are few bright spots in Sloan's attention to economics and in the Russell Sage Foundation's programs but I am sure others will speak of these and I need not dwell on them. Newer foundations have brought important stimuli, as MacArthur has done for nternational security and environmental studies, but there has been no evidence of a new general interest in the basic disciplines as such. Nor does one readily foresee a shift of policy in the older foundations in this direction.

There is certainly no dearth of questions and concerns about the state of the behavioral and social sciences that need to be addressed. The growth of the fields and the professional communities in them has been enormous in the past generation and has brought such dizzying change that the journals burgeon with efforts to say what the fields now are. Clifford Geertz has recently tried to tell what the field of anthropology has become since the old days when it was comfortable as a holding company for physical anthropology, linguistics, archaeology, and social and cultural anthropology. The old boundaries have burst at many points, the old objects of study in tribes and exotic communities have dissolved into the provinces that belong to other disciplines, and distinctive methodology becomes an uncertain refuge. With characteristic adroitness, Geertz eludes despair by observing that anthropologists, whatever they are, still win the attention and esteem of wider intellectual communities,[6] but he leaves much for others to dispute about his field.

Evidence of similar disquiets in sociology caught public attention recently in the rejection of an appointment at Harvard on the grounds that it would not serve the core of the discipline. Struggles over the accommodation of new talents, new subjects and new methods can certainly be viewed as signs of healthy growth. They are nonetheless perturbing and provoke conflicts that many have found dispiriting. The immense variety of activity that aspires to be important and authoritative inevitably requires a sorting and evaluation in any field that aspires to the name of a discipline. The social and behavioral sciences, no less than others, must have agreed standards of reliable knowledge and of what is important. The struggle to maintain these standards without throttling variety and innovation is perennial and must be the responsibility of leaders in the discipline. It is not a process that can be legislated or controlled by outsiders. Beyond an uncertain minimum, the effects of money are probably secondary. But lack of money for developments that look to be important for a discipline can seriously reduce the flexibility and geniality that ought to prevail as the leaders of disciplines balance contending standards and claims. The present dearth of foundation attention to matters that are important to the disciplines and not merely to their uses certainly increases the importance of the Federal government's

---

6) Times Literary Supplement, June 7, 1985, pp. 623-624.

support of these fields through the NSF, the NIH and other agencies. As Kenneth Prewitt has frequently emphasized during his years as president of the Social Science Research Council, there is need for a kind of national general staff worrying about the state and needs of the social and behavioral sciences. One of the losses from the decline of general foundation concern with these fields has been a diminution in the small band of people and organizations that have fulfilled this function.

Looking to the future, there may be some glimmerings of prospect that the foundations may be moved toward a broader concern for these fields. I often think we have been passing through a time reminiscent of the Romantic Revolt at the beginning of the nineteenth century. There came in the turbulence of the late 60s and early 70s a passion for direct experience and a reaction against rational deliberation that was like the turn against the grand visions of the Enlightenment. We still feel the force of this tide and I see in the programs of foundations more taste for action programs and the complementary distaste for "studies for the shelf" than was characteristic a couple of decades ago. There has even been a decline in enthusiasm for policy studies that has strengthened the taste for action and demonstration programs. But tides that rise also fall, and we will undoubtedly think differently in years to come. What our next hopes and enthusiasms may be can only be dimly discerned but there is reason to think that they may again embrace the social and behavioral sciences more warmly. Some of the disillusionment with these subjects that came along in the last decade and a half was a result of overconfidence. There was a naive positivism that marked much of what we did, domestically and internationally in the years after World War II. We knew better than to believe that right knowledge would suffice for dealing with human problems. A former president of the Social Science Research Council told a Congressional Committee in 1954 that "the social sciences provide no complete answers to any practical problem."7) But in the heady self-confidence that marked the American ascendancy after World War II, we tended to forget such cautions. Now, in a more chastened mood, there is a prospect that we may be ready to tip the balance away from ready solutions and pursue deeper understanding. One sees much evidence of this tendency in the more amicable relationships between the social sciences and the humanities that now prevail. This rapprochement seems to come, in part at least, from a renewed sense of the manifold diversity of human experience and the need to recognize its concreteness and complexity without abandoning the order of theory and generalization. Whether we are trying to know what to make of militant Islam or the changes in family life in this country, there is an enlivened appreciation that we must understand a great deal that we can do little about if we are sensibly to select what we can do. The studies which envelop sensible courses of action must come from people trained in the ways of objective, disinterested knowledge. Certainly not all of them will be professional social and behavioral scientists but nearly all will be touched by these subjects.

---

7) Statement prepared for the Reece Committee, June 15, 1954, supplementary statement B, p. 8, by Pendleton Herring.

In such a future, it is reasonable to expect more vigorous and comprehensive attention by the foundations to the social and behavioral sciences. The diminished strengths of the foundations vis-a-vis government and indeed many other parts of our society mean that the foundations are not likely ever again to assume the prominence they had a few decades ago. The role of governmental support--particularly from the Federal government, but also from the state governments through their support of universities and research activities--must be of critical importance in the future. On the other hand, the diminished scale of the foundations provides attractions for them to play intellectual and innovative roles. Foundations are not likely to abandon their efforts to foster experiments and innovations. But the reach of new knowledge and ideas and the need for varied sources of support to those who produce them have lasting attractions that one dares hope will grow in the future to the benefit of our social sciences and our society.

Mr. WALGREN. Thank you very much, Dr. Sutton. We appreciate that presentation.

Let's then turn to Dr. Rees, and we'll go down the line.

STATEMENT OF DR. ALBERT REES, PRESIDENT, THE ALFRED P. SLOAN FOUNDATION, NEW YORK, NY

Dr. REES. Thank you, Mr. Chairman. I am Albert Rees. I am the president of the Alfred P. Sloan Foundation. Before I came to the foundation, I was professor of economics at Princeton University and at the University of Chicago. Like Dr. Sutton, I am speaking in a personal capacity and not for the trustees of my foundation.

Sloan is an endowed private foundation that makes grants in economics, management, science and technology. We are among the 20 largest private foundations, but we are a lot smaller than Ford. The last figure in Dr. Sutton's table showed that Ford made total grants in 1984 of $167 million. Our total grants in that year would be about $20 million, so it's roughly a sixth or a seventh the size of the Ford Foundation.

Nevertheless, we are important in two areas of the social sciences. One is economics, where we have had a program for a very long time, and the other is cognitive science. We don't support the social and behavioral sciences across the board. We have nothing much in sociology or political science, for example. In economics, we think that we are the second largest supporter of American academic economics, after the National Science Foundation. The Ford Foundation and possibly the Rockefeller Foundation do more to support economics overseas, development economics and agricultural economics in Third World countries, than we do, but we probably are second only to NSF in our support of economics as an academic discipline in major American research universities, and that is done largely through support of graduate students through fellowships and support of junior faculty members.

Nevertheless, to give you some idea of the scale, our program is most similar to the NSF's program in economics, but it is less than a third the size and, as Dr. Sutton has indicated, the number of private foundations that are supporting the social and behavioral sciences is not large and it is not increasing. Most private foundations are engaged in very different kinds of activities, such as supporting the arts and humanities or relieving human distress through clinics and food banks and hospitals and things of that sort.

Now with the funds for social science research so limited, I think it is very natural for the private foundations to try to carve out a special role for themselves. We have tried to do that, and I want to indicate what some of those special roles are. One of them is to search for areas of research that are not yet well enough established to be funded on any substantial basis by the Federal funding agencies, and to help those new areas of social science develop and grow to the point where they are recognized by the Federal funding agencies and can attract Federal support. It is a sort of intellectual venture capital.

This has been done with conspicuous success, I think, in the new interdisciplinary field of cognitive science by two foundations, first Sloan and more recently the Systems Development Foundation. Both of these private foundations will soon withdraw from the field of cognitive science, in the hope that it is firmly enough established intellectually so that it will attract long-term Federal funding. Indeed, the Federal funding agencies are considering the support of cognitive science, but they are not yet organized in such a way that cognitive science as a disciplinary field has a logical home within the funding agencies. That is, there is no particular, say, study section in NIH that would say, "It's our mission to support cognitive science." Those study sections are pretty narrowly specialized.

Now another role of the private foundation that we don't play perhaps as often as we should is to support the unorthodox scholar, the scholar whose means of the mainstream to make it difficult for him to receive high marks in the formal peer review process. We occasionally make risky bets of this kind, and with all risky bets some are losers, but some are winners and will lead to stimulating and original research. I am proud to give as an example of that, that the Sloan Foundation supported the book that has just come out on crime by James Q. Wilson and Richard Hernstein. I haven't finished reading it yet, but I think it is going to be an interesting and a controversial book, and it's not the sort of thing that probably would have gotten support from the peer review process.

A third area in which private foundations may have an advantage is in funding applied research that appraises public policy in areas of political controversy. The advantage of the foundation in an area of political controversy is that it is not beholden to anyone. It doesn't have to account to voters or to members or to stockholders if the outcome of the research it supports proves to be unpopular or offends some powerful interest group. A classic model of this kind of appraisal is Gunnar Myrdal's study, *An American Dilemma* (1944) which was funded by the Carnegie Corp. of New York.

But although our freedom from special interest pressures is an advantage in principle, it's not always easy to apply in practice, and I don't believe that we have yet found a generally successful device for organizing and staffing large-scale appraisals of major areas of public policy. For this reason, I don't think such appraisals ought to be a major part of private foundation activity in funding the social sciences. This term "appraisals" is a term of art which I believe was coined by Aaron Wildavsky when he was president of the Russell Sage Foundation.

One area of funding in the social sciences where the private foundations can't play a major role is in the acquisition of data. A great deal of social science research is based on economic and social statistics that are provided by Federal Government agencies or with the help of Federal Government funding. The Bureau of the Census, the Survey Research Center at the University of Michigan, and the National Opinion Research Center at Chicago play the same role in the social sciences that large telescopes play in astronomy or that particle accelerators play in high energy physics. The routine support of large-scale data collection is simply beyond the resources of the few private foundations that are interested in social science research.

The Federal budget for fiscal year 1981 largely eliminated National Science Foundation support for collection of data for the Panel Study of Income Dynamics, which is a unique longitudinal data base that at that time had tracked the income and employment of the same 5,000 American families for 14 years. That data base is very widely used by economists and sociologists at scores if not hundreds of universities, and it's invaluable for answering a lot of questions, such as whether the poor are the same set of people from year to year—that is, whether we have a permanent underclass of poor—or whether there is turnover in this group. The answer turns out to be that there is a good deal of turnover. You can only find that out from a longitudinal data base.

Now if the collection of the Panel Study on Income Dynamics had been interrupted for a year, they would have had to go back to square one and start all over again. Fourteen years of continuous data would have been broken, and you would have had to start again. It would have taken 14 more years to get to where you were in 1981, and that was zeroed out in the 1981 Federal budget originally. Eventually some small portion of the funding did come from Federal sources.

Well, I'm glad to say that the continuity of that data base was preserved through grants to the University of Michigan from three private foundations, Ford, Rockefeller, and Sloan, and the remaining fourth was eventually provided by NSF from funds that they scrounged up somewhere. Others in the room are better able to tell that story than I am.

But my point in telling this story is that you shouldn't count—Congress shouldn't count and OMB shouldn't count—on that happening again. It's not reliable. The next time that OMB and the Appropriations Committee enact the "Perils of Pauline," the heroine may fall in the gorge. Adequate, continuous, and assured support for basic data collection is the most important single way in which the Federal Government can support the social sciences, and the second most important, I would say, is the provision of research funds through the National Science Foundation through the peer review process, and then finally you have the funding of applied research through the mission-oriented agencies, which is very important but which is not as important to the social sciences as academic disciplines as it is to the mission of the agencies themselves.

Now we all know that the social sciences have not yet approached the levels of the natural sciences in their ability to test hypotheses and to establish generally accepted theories, and they probably never will be. It is inherently easier to predict the return of Halley's Comet than to predict the behavior of human beings, either as individuals or as societies. That particularly is true of my discipline of economics, which has got a very bad record of being able to predict anything. [Laughter.]

Moreover, the titles of social science research projects, which use everyday English language rather than highly technical scientific terms, sometimes provide easy targets for uninformed criticisms. Some of the leading uninformed critics are in the Congress, but I'm glad to say that they're not in this House.

Nevertheless, there are many areas of the social sciences in which there is a substantial body of accepted theory in which good progress is being made and in which research findings have great relevance for public policy. I might just cite one case in which I think some social science research findings have tremendous relevance for public policy.

The Congress is now considering whether to let the tax on cigarettes go down from 16 cents a pack to 8 cents a pack. Harvard University has an Institute on Smoking Behavior in the Kennedy School, headed by Tom Schelling, a very able and versatile economist. They have done some wonderful studies that show that the increase in the cigarette tax from 8 cents to 16 cents, while it had very little effect on the behavior of confirmed smokers, had a demonstrable and large effect in preventing teenagers from taking up smoking in the first place. It seems to me that that information, which comes from social science research, will be very useful to the Congress between now and the first of October.

Well, despite the fact that we are not as scientific as physics or chemistry or astronomy, I think a good case can be made for continued support both from the Government funding agencies and from the private foundations. Thank you, Mr. Chairman.

[The prepared statement of Dr. Rees follows:]

TESTIMONY OF
ALBERT REES

BEFORE THE TASK FORCE ON SCIENCE POLICY,
HOUSE COMMITTEE ON SCIENCE AND TECHNOLOGY

September 19, 1985

Mr. Chairman, I am most pleased to have this opportunity to testify before this Task Force. My name is Albert Rees; I am President of the Alfred P. Sloan Foundation; and I appear this morning in a personal capacity, not to speak for the Trustees of the Foundation.

The Sloan Foundation is an endowed private foundation that makes grants for higher education and research in science, technology, economics and management. In the social and behavioral sciences, our activities are concentrated in two areas; economics and cognitive science. I should like to express some views on the roles of the Federal government and of private foundations in supporting research in the social and behavioral sciences.

The Task Force should understand that the scale of private foundation support of research in the social sciences is quite small relative to the scale of Federal support. We believe that the Sloan Foundation is the second largest supporter of American research in economics after the National Science Foundation, but our annual level of grants in economics is under $3 million dollars, which is less than a third of the amount spent directly on economics by NSF. The number of private foundations that support research in the social sciences is also very small and is not increasing. The vast majority of private foundations are engaged in equally worthy but quite different tasks, such as the direct relief of human distress through settlement houses, clinics, or hospices. Among the foundations that do support social science research, most have focused on research with direct policy relevance, and devote little of their funds to basic research into the nature of human behavior.

With private funds for social science research so limited, it is natural for private foundations to try to play a special role. One of our special tasks is to seek out promising areas of research not yet well enough established to secure regular support from Federal agencies, and to help these new areas develop and achieve recognition. One could say that we are providing intellectual venture capital. This has been done with conspicuous success in the new interdisciplinary field of cognitive science by the Sloan Foundation and the Systems Development Foundation. These private foundations will soon withdraw from this field in the hope that it is firmly enough established intellectually to attract long-term Federal funding. Indeed, Federal funding agencies are currently considering the support of cognitive science, but they are not yet organized in such a way that this interdisciplinary field has a logical home.

Another special role of the private foundation, which perhaps we do not play as often as we should, is to support the unorthodox scholar, whose methods or views are enough out of the main stream to make it difficult for him to receive high marks in the formal peer review process. We occasionally make risky bets of this kind and, as with all risky bets, some are losers, but some produce original and stimulating work that opens new avenues of research.

The third area in which private foundations may sometimes have an advantage is in funding applied research that appraises public policy in areas of political controversy. The advantage of the private foundation in an area of controversy is that it is not beholden to anyone. It does not have to account to voters, members, or stockholders if the outcome of the research it supports proves unpopular or offends some powerful interest group. A classic model for this kind of appraisal is Gunnar Myrdal's An American Dilemma (1944), funded by the Carnegie Corporation of New York. Yet although foundations' freedom from special interest pressures is a powerful advantage in principle, it is a difficult one to capitalize on in practice. I do not believe that we have yet found generally successful models or devices for organizing and staffing effective appraisals of public policy. For this reason, such efforts ought not, in my opinion, be a major part of private foundation activity in funding the social sciences.

A major area of funding of the social sciences where private foundations are not and cannot be very active is in the acquisition of data. Much social science research is based on economic and social statistics provided by Federal government agencies or with the help of Federal funding. The Bureau of the Census, the Survey Research Center at the University of Michigan, and the National Opinion Research Center in Chicago play the role in much of social science that large telescopes play in astronomy or that particle accelerators play in high energy physics. The routine support of large-scale data collection is simply beyond the resources of the few private foundations interested in social science research.

The Federal Budget for Fiscal 1981 largely eliminated NSF support for the collection of data for the Panel Study of Income Dynamics, a unique longitudinal data base that at that time had tracked the incomes and employment of the same five thousand American families for fourteen years. This data base is widely used by economists and sociologists at scores of major research institutions, and is invaluable for answering such questions as whether the poor are the same people from year to year, or a changing set of people. Interruption of the collection of this data would have destroyed the continuity of the series. A new longitudinal sample would have had to start over again from square one.

I am glad to say that the continuity of the Panel Study of Income Dynamics was preserved through grants to the University of Michigan from three private foundations, Ford, Rockefeller, and Sloan and that the Panel is now once again fully funded by NSF. My point in telling this story is to warn that the Congress cannot count on such rescues from private foundations to take place regularly--the next time the budget committees enact the Perils of Pauline, the heroine may fall into the gorge. Adequate, continuous, and assured support for basic data collection is the most important single way in which the Federal government can support the social sciences.

We all know that the social sciences have not yet approached the levels of the natural sciences in their ability to test hypotheses and establish generally accepted theories. They probably never will. It is inherently easier to predict the return of Halley's comet than to predict the behavior of human beings, either as individuals or as societies. Moreover, the titles of social science research projects, which use everyday English terms rather than highly technical language, sometimes provide easy targets for uninformed criticism. Nevertheless, there are many areas of the social sciences in which there is a substantial body of accepted theory, in which good progress is being made, and in which research findings have great relevance for public policy. Continued support of social science research by both government and private sources is therefore warranted even in periods of budgetary stringency.

Mr. **Walgren**. Thank you, Dr. Rees, very much.

Dr. Etzioni.

[A biographical sketch of Dr. Etzioni follows:]

Dr. Amitai Etzioni

Dr. Amitai Etzioni is the first University Professor of The George Washington University. He served as Senior Advisor in the White House from 1979-1980. He was guest scholar at the Brookings Institution in 1978-1979. For 20 years (1958-1978), he served as Professor of Sociology at Columbia University; part of the time, as Chairman of the department.

He is the author of fourteen books, including *A Comparative Analysis of Complex Organizations, Modern Organizations, Political Unification, The Active Society, Genetic Fix, Social Problems,* and most recently, *An Immodest Agenda,* and *Capital Corruption.*

Dr. Etzioni's achievements in the social sciences have been acknowledged by several fellowships, including the Social Science Research Council (1960-61), the Center for Advanced Study in the Behavioral Sciences (1965-66), and Guggenheim (1968-69).

Outside of academia, Dr. Etzioni's voice is frequently heard in the press, including *The New York Times and* and *The Washington Post.* Etzioni has presented social science perspectives on *The Today Show, 60 Minutes, A.M. America, Firing Line,* and *The Phil Donahue Show.*

In 1983 Dr. Etzioni has been appointed to the Economic Forum of the Conference Board. Corporations Dr. Etzioni has consulted for, or provided presentations to, at the corporate level, include Allied Corp., Aetna, AT&T, Bethlehem Steel, Bristol-Myers, Quaker Oats, Pfizer, and Prudential. He has consulted widely for government agencies, including the Departments of HHS, Labor, Commerce, and the Treasury; for the N.S.F., the President's Commission on the Causes and Prevention of Violence, and the White House during several administrations. He has testified frequently before Congressional committees.

He founded the Center for Policy Research, a not-for-profit corporation dedicated to public policy, in 1968, and has been its director since it inception.

In 1976, the American Revolution Bicentennial Administration accorded Dr. Etzioni a certificate of appreciation for his outstanding contribution to our nation's bicentennial commemoration. He was awarded an honorary degree by Rider College.

A 1982 study ranked Dr. Etzioni as the leading expert of 30 who made "major contributions to public policy in the preceding decade."

Dr. Etzioni is married and has five sons.

STATEMENT OF DR. AMITAI ETZIONI, UNIVERSITY PROFESSOR, GEORGE WASHINGTON UNIVERSITY, WASHINGTON, DC; AND DIRECTOR, THE CENTER FOR POLICY RESEARCH, BETHESDA, MD

Dr. **Etzioni**. I am Amitai Etzioni, university professor, George Washington University, and director of the Center for Policy Research. Mr. Chairman, with your permission, I will submit the written statement for the record and just deviate from it in a few comments.

My main argument is that, aside from strengthening the social sciences and guaranteeing their continued support, very much along the lines of what we just heard, which I fully support, I am particularly concerned with the steps we need to take to increase the policy yield of the social sciences as they are and as they continue to be strengthened. I believe an analog will help to make the point that only strengthening the social sciences, which we need to do, will not automatically by itself increase the yield for policy studies and policymaking.

The analog I would like to draw on is taken from another profession, as distinct from discipline, that of medicine. If one of us takes ill, we would be extremely ill-advised to draw on a panel of the leading scientists in the field. If you could have your treatment being prescribed by a Nobel Prize winner in genetics, physiology, and other related disciplines, you would do much less well than with a lowly internist. The question is why.

The basic reason is that the sciences, as they ought to, specialize. They slice the phenomena and they fragment it, and then zero in each one on one aspect of it, in the social sciences either the personality or the economy or the society or culture or some other such thing, which is exactly the way it is supposed to work. Without division of labor, none of them could carry out their duty. As you already heard, it is difficult enough even when you just deal with one slice of the phenomena. But policy is inevitably in the multifacted world, and attempts which have been made in the past when we move directly from one science to intervention in the system, fly in the face of the fact that all factors, all main factors, at least, must be moved simultaneously or those which are not moved come to haunt those which have been moved.

Now what the medical profession does is really to use three procedures to deal with the problem. It is extremely difficult to combine knowledge from different disciplines. They do draw on a multiplicity of disciplines, rather than one at a time, and train people and provide systematic preparation for combining psychology with genetics, psychology with physiology, and things like this.

Second, about 50 percent of the prescriptions in medicine cannot be traced to any analytical input at all. They are just good practice which has been transferred from one generation and group of physicians to another because we know it works, although we often don't know why. By the way, the same holds for other professions. Engineers have formulae to calculate metal fatigue, how thick they should make the cables on which to hang the elevators, but I am told that they usually make them twice as thick as the formula tells them because practice tells them that if they should rely on the formula, elevators would tend to fall, and so you would rather err on the side of safety than rely on the mathematical formulae. So about half the knowledge is transmitted in the practice, not simply by practitioners but by people who study practical procedures.

Finally, and maybe most interesting and most important, there are special procedures which you apply when you don't fully know what you are doing, so the reason a typical physician prescribes a medication and then asks the patient to come back after 3 days is because he is not sure it's going to work. If he would be quite sure that the medication will work, there was no need for the patient to come back, and then a second reading is taken or whatever it is—the content of the blood, the temperature—and then usually the dosage is adjusted, and if that doesn't work a different medication is tried.

This is not a random trial and error, the kind you use when you don't know what you're doing. You start with a working hypothesis, what is going to work, but also fully realizing that you will have to keep adjusting your intervention as additional knowledge is acquired, not derived from your prior knowledge but from experience as you proceed, so you analyze your experience and keep correcting your intervention.

These are the three things—piecing together different sciences, using applied practical knowledge, and using special procedures to hedge against the fact that you know only part of what you need to know—which characterize medicine and all other professions. Now when it comes to social science we have begun—and it is very important—over the last 15 years but not much before, to concern ourselves systematically with the questions of how we move from important work done in economics, in psychology, et cetera, to prescribe policy interventions.

Places like the Kennedy School, the School of Public Policy at Berkeley, and increasingly other places do now do the equivalent of what medicine does and study the problems and procedures of application, though we have a long way to go and if you scratch these places, you will find very often that because it is a difficult process at this stage to make the transition, that they fall back on simply teaching analytical sciences. As a result, the preparation is often excellent in economics or psychology or political science, but we don't get the kind of synthesis and preparation you get in other professions.

That has a direct application, now, to the mission-oriented agencies which were already mentioned, and one of the few recommendations I would like to make is that if another hearing could be conducted or if one of the appropriate bodies, either OTA or Congressional Research Service or some other—GAO—could look into the matter of what happens to the mission-oriented agencies when they try to apply social sciences. There are many institutional reasons why they tend to move in the direction of, in effect, doing basic research.

Now I walk here on thin ice, I should say. I am not for a second questioning the significance of basic research. I mean, it used to be like challenging motherhood, and I guess over the years we learned to deal with contraceptives, so I have a similar problem here. Let me then emphasize I am in favor of motherhood, my mother being here and my wife, and five children--they are not here—and I am in favor of basic research, but we have come to the appropriate place where we need to worry about the other part of the story. The question is if in the appropriate places we are applying the procedures and training the kind of people and retaining the kind of people who have the applied skills. Otherwise you get in a vicious cycle where, because the mission-oriented agencies support and retain basic researchers, then we don't train applied researchers and then they cannot find them. That's something I think deserves some attention.

I am not saying that everybody in the mission-oriented agencies is basic research oriented, but it's something that deserves some study. Again, it is not a question of evil intention. The transition is often very, very difficult and the campuses have not given the full support they could have given to develop the skills the mission-oriented agencies could use. But, as I say, somewhere this cycle has to be broken into, and probably one place that could be done is in one or two of the mission-oriented agencies.

I should mention in passing that we have here an in-and-out problem with which some of you may be familiar from the Defense Department, that we have academicians working for mission-oriented agencies, fully aware they want or will return to academe, and you don't get in academe many points for having solved the problems of the Department of Agriculture or Energy or whatever, but if you build a nice model which leads to a new mathematical variation or can be published in the appropriate academic journal, then indeed you can return to the campus with a few extra points. Having done so myself, I think I know what I am speaking of.

Therefore, in the longer run the question will be how as a parallel on the one hand to continue support for strengthening the basic social sciences, without which we don't have the foundation for application, and at the same time somewhat enriched opportunities for application.

Just to close here, there are then indeed systematically different canons for research when you seriously take on the mission of policy research as distinguished from basic research, and let me just list some of them so the distinction will be somewhat clearer. The most important one by a long shot is the distinction between dealing with the system as distinct from any analytical element of it, the difference between dealing with a subset of variables, economic or social or psychological, and the need to concern yourself at least with all the major factors which affect the particular phenomena in hand. That's probably the single most important distinction.

The other distinction is the difference in relation to timeframes in decisionmaking units. The work of science is never done, and an honest scientist will tell you that he or she is not aiming at finishing—they may want to finish their project, but not science—on Monday or at the next hearing or the next time a policy has to be formulated. It is going on for several hundred years and, God willing, they will go on forever. So what we all do, we are ants—some of us are large ants—and we add a little piece to the accumulating pyramid we hope we eventually will construct.

Those who work in the policy process, if the input is 2 weeks after the legislation has been passed or they analyze the program—as has happened, by the way, quite often—after it has been either canceled or approved, they are somewhat out of step with the policy process, so the first consideration for a policy researcher is, when is the next decision point? Research has to be designed such that it be ready at that point. That forces you to use different procedures.

Just to give one example, sociologists are fond of using survey research, which often takes a year or better and it is often funded at 6 digits, as it ought to be, and it allows you to study things in depth and responsibly. However, we have also something known as a minisurvey which you can do for a small fraction of the cost in 3 weeks. It does not give you anything like the depth and integrity of the data a full survey does, but it gives you a very good first approximation, give or take 15 or 20 percent, for what you want to know.

The question often is, "Is this procedure"—let's say putting a tax on gasoline. For a long while, people in Congress used to believe the public would be outraged, would not sit for such a thing. Well, a minisurvey would have shown that the public was quite understanding of the need to do so.

We may not have nailed it down to the last 7 percent, but we would have reported that somewhere between 70 to 80 percent of the public, give or take 10 percent, 15 percent, were quite supportive. That is but the many examples of the difference in research approach you use when you want to have less reliable, less depth information in time for the policy frame, than when you have the longer breadth as the basic researchers ought to have and ought to continue to have. Similarly there is the question of sensitivity to the constraint of the decision maker. It is a tenet of social science, of course, that when we do basic research we ought not to be and are not limited by what is at the moment politically or economically feasible. If it makes sense analytically we ought to study it. Policy research obviously will have to take into account the kind of constraints the policymakers are working on and not explore at great lengths options which are politically and economically at the moment completely out of the question.

I could go on, but I think I made my main point. I would favor setting aside a small part of the resources, 5 or 7 percent, to be clearly dedicated to increasing the policy yield of social science, to develop the skills in that area, and I would like to see some examination of what factors in the mission-oriented agencies hinder, if they are, the greater use of social science.

Thank you very much.

[The prepared statement of Dr. Etzioni follows:]

THE NEED FOR, AND WAYS TO ENHANCE POLICY SCIENCE CAPACITY OF SOCIAL SCIENCES

Testimony before the

COMMITTEE ON SCIENCE AND TECHNOLOGY
Task Force on Science Policy
"The Role of the Social Sciences"

September 19, 1985

Amitai Etzioni
University Professor
714J Gelman
The George Washington University
Washington, DC 20052

Director
The Center for Policy Research
PO Box 34595
Bethesda, MD 20817

THE NEED FOR, AND WAYS TO ENHANCE POLICY SCIENCE CAPACITY OF SOCIAL SCIENCES

There is a need to develop the policy capacity of the social sciences above and beyond their analytic capacity. The use of sciences to change the world, the purpose of the policy, is quite different for their use to merely understand the world. (The distinction is sometimes captured in terms of the difference between analytic disciplines and the professions, whose purpose is to render service to patients, corporations or society, such as medicine and engineering). We have a strong social science disciplinary base, although it too needs additional support, so it will continue to be expanded and strengthened. However, the social sciences' policy capacities and resources are relatively under-endowed. Indeed, until fifteen years ago there was very little social science policy training. Policy schools, such as the Kennedy School at Harvard and the Graduate School of Public Policy at the University of California at Berkeley, are quite recent creations and much of what they teach is still analytic social sciences.

To encourage the development of the policy capacity of social sciences I suggest that a given proportion, say of 5 percent to 7 percent, of the federal budget for social sciences will be set aside for the development of the policy capacity and that a few centers will be funded to focus on developing this capacity; that the GAO, the Office of Technology Assessment, and the CRS will be asked to analyze the social sciences' policy capacity of mission-oriented federal agencies, and that hearings on these matters will be conducted. (In my experience, many of the social scientists working for mission-oriented federal agencies, are not trained in policy research nor are their supervisors and their work).

How do policy sciences differ from analytic sciences? First and foremost, policy sciences draw on encompassing models as distinct from highly specialized ones. Analytic sciences proceed by fragmenting, by focusing on a limited sub-set of variables, say economics or social or psychological ones. Policy researchers and analysts must deal with all the main factors, because the policy realm is affected by all of them simultaneously. It is not possible to move some factors while ignoring the others that impinge on the same phenomena. In contrast, the analytic sciences can hold constant other factors or assume that they are unchanging.

Second, the policy researchers and analysts must take into account that various factors, "represented" by different analytic disciplines, not merely all affected the phenomena under study simultaneously, but also -- interact with one another. Hence, for example, in a study of the effect of changes in inflation rates on saving behavior we must study not only consequences of changing interest rates and of changing expectation about future prices, but also the effects of changing expectation of prices on the levels of interest rates. The study of interaction effects raise numerous technical problems, policy analysts and researchers must learn to cope with.

Other differences between policy and analytic approaches are less pivotal but far from trivial. Analytic science views, quite appropriately, the labor of science as never ceasing, and hence not tied to any particular time framework. Policy analysis is often tied to specific decision points and hence to limited time units. The product of analytic science is a publication; the product of policy science is a briefing, often not open to others. Analytic science is free to explore all avenues; policy science -- must be sensitive to the constraints under which its clients labor.

We need strong, even stronger, analytic social sciences because they are _the_ foundation on which social science policy research and analysis builds. However merely strengthening the social sciences _per se_ will not obviate the need to develop the policy capacity. This need must be attended to separately, just as the development of the life sciences by itself has not sufficed, we need -- in addition -- to enhance medicine, which uses these sciences in the treatment of patients. _The application often does not flow automatically_ and the ability to use a mix of sciences to solve a problem requires special training, a social science policy profession.

Amitai Etzioni is University Professor at the George Washington University and Director of the Center for Policy Research. He is the author, recently, of An Immodest Agenda: Rebuilding America Before the 21st Century. For more discussion see "Making Policy for Complex Systems: A Medical Model for Economics," Journal of Policy Analysis and Management, Vol. 4, No. 3, 1985, pp. 383-395.

Mr. WALGREN. Thank you, Dr. Etzioni. Thank you.
Dr. Pew.

STATEMENT OF DR. RICHARD W. PEW, PRINCIPAL SCIENTIST, BOLT BERANEK NEWMAN LABORATORIES, CAMBRIDGE, MA

Dr. PEW. Mr. Chairman, members of the task force, I am Richard W. Pew. I am a principal scientist at BBN Laboratories—Bolt, Beranek & Newman—which is a publicly-held R&D firm in Cambridge, MA. I moved to Boston in 1974, after 11 years as a professor of psychology at the University of Michigan. I am currently president of a division of the American Psychological Association called the Applied Experimental and Engineering Psychology Division. For 25 years I have been involved in teaching and research and development and consulting concerning the applications of psychology to system design, both in the university and in an industrial laboratory.

I am not representing any particular organization. I am sort of representing myself, but I think I speak for the professionals in the area. We don't have any exact estimate of the number of such professionals, but if you extrapolate from the membership in relevant societies, I would say it is probably 5,000 to 7,000 individuals that are actually practicing in this domain. They go by various names: ergonomics, human factors engineering, engineering psychology, cognitive engineering, applied experimental psychology, and lots of different names.

It's an area that is often overlooked when one thinks about potential applications of psychology, and in that sense perhaps what I have to say belongs in part in yesterday's presentation rather than in today's, but I believe it is an area that is both underfunded and without an adequate home outside of the military context. I'll say something about that in a moment.

It is also an area that is receiving considerably increased public attention. We have had the Three Mile Island incident, and we have had now a number of chemical spills threatening the public. In those situations those coming from my background would ask the question, "Where are the accident-provocative situations?" That is, rather than looking for a particular human error, we would look for issues related to the design of those systems that make it likely that an error would occur. It's a matter of looking at the design from the perspective of improving the safety from the point of view of the user or the human operator.

In military systems there is much discussion of the fact that many systems are failing because they are difficult to use and are difficult for the user to understand, and there are problems of maintenance. There is just as much an issue in design for maintenance as there is in design for use.

In the commercial domain we are designing computers and computer applications for the home, for the school, and for the workplace, and all of these systems need to be thought about from the perspective of the human user.

There are, in addition, other Government agencies that are beginning to have the need for application but only rarely understand exactly what the issues are. I had a project with the Social Security Administration some years ago concerned with implementing computer systems for use in district offices. Only today are they beginning to move forward with some developments of that kind. The IRS is talking about applications of high technology in some of their systems, and even an area like the U.S. Weather Service. Basically what the Weather Service does is apply human judgment to a vast array of sources of data, and the technology for how to bring that judgment to bear in order to improve the success of prediction can have a very significant impact in agriculture and in industrial settings.

Let me speak for a moment about the kind of research needed. The practice of human factors or engineering psychology is often said to be common sense, and in fact each of you can probably think about cases where you see a failure of design in your own daily activities. I have been an expert witness in products liability cases where needless accidents have occurred because of something that certainly superficially appears to be common sense, where the designers did not foresee what turned out to be a very common mode of operation that turned out to be unsafe.

So what you need to do is to put yourself in the designer's position. What he needs to be able to do is to predict what is going to be common sense before a product has been built, before there are a lot of them out in the field. It was commented that a model for medical practitioners is to make a prescription and then to ask the patient to come back in 3 days and see if it worked. Well, the discipline that a psychologist would use is that he would make a prescription and then, if he was uncertain about its outcome, he would do an experiment, but in industrial settings there are rarely the resources or the time available to do an experiment that is needed to prove out the design prior to its use.

Under those conditions it means it just heightens even more the need for good science and good theory on which to make these "common sense" predictions. The prediction not only has to be before it is built; it has to be for a wide range of users of different backgrounds, not just for you. Further, he is making decisions that have to be mutually consistent with a lot of other decisions. In this kind of systems context we have to take all of the possibilities together.

Therefore, basically common sense after the fact requires a lot of knowledge prior to design, and it's that kind of knowledge that we need to develop further. We need to be able to predict a user's behavior with respect to equipment. We are concerned with human physical, physiological, cognitive characteristics, human preferences, human performance capacities and limitations.

I would like to draw the analogy with a kind of engineering model. In Pierre Boulet's book, *The Bridge on the River Kwai*, where they are into a process of building a bridge over a river in Asia, he talks about how the sort of Western society deals with this problem by thinking about all of the shapes of the beams and the depths to which the piles are driven and so forth prior to design. Then he goes on to say, "These figures in their turn depend on coefficients worked out according to standard patterns, which in the civilized world are given in the form of mathematical tables. Mechanics, in fact, entail a complete a priori knowledge, and this mental creation which precedes the material creation is not the least important of the many achievements of Western genius." It's true we add a safety factor of two or four to those predictions, but the predictions form the foundation for that kind of understanding.

What I am suggesting is that we are in a state where the basic sciences are beginning to provide us with the kind of information that makes it possible to build engineering models that relate to human behavior. If I can impose on my colleague here for just a moment, I'll give a very simple demonstration. Would you hold up your finger? For the record, hold up your finger opposite my finger, one of yours. I am going to move my finger in an irregular pattern, and I want you to follow my finger as closely as you can. Move your finger as I move my finger. You don't do that very well. [Laughter.]

If I know the equation of motion of my finger, I can predict the equation of motion of the user's finger, if you like, and while we can't do perfectly at making such a prediction, if the user is highly motivated we can make a reasonable prediction. It's a kind of a model that is applicable to automobile driving, to flying an airplane, and a number of other areas where we can begin to solidify predictions and begin to narrow the range of things that need to be tested, and it's a very important area of development in psychology and in the relationship between psychology and engineering.

So my thesis basically is, except in isolated instances the data and models that are needed in order to carry out this kind of practice are not available or are available to only a limited extent. I want to sharpen the kind of research that is really needed. I am not talking in this particular instance about the kind of basic science that is undertaken in universities, that is generally generated by investigator-initiated grants. Again, I am not in any way belittling the importance of that. That's very important, and we couldn't begin to be doing the things we are doing if it weren't for the basic science support. On the other hand, it is not the highly specific mission-oriented development that will improve a specific system, of the sort that is done in most military contexts. The military establishment supports a great deal of work that is oriented toward the design of specific equipments and tries to take account of human factors principles.

It's kind of in the technology transfer area in between those two. The need arises in an applied practical context, but the kind of data that one needs to obtain are generic. The data are more general than any particular application, and it's that class of more general data and the models that are derived from that data that is the area that I believe is being neglected. It is, in a sense, falling in the crack perhaps because it's interdisciplinary. It isn't really psychology in the sense that many psychologists think of it, and it's not really engineering in the sense that many engineers think of it, but it's clearly right in that interface between those two disciplines.

For example, the same kinds of principles that are needed to sort of correlate the judgment of the weatherman is the same kind of data that would be useful or that is useful in helping a radiologist to improve his diagnostic accuracy. In fact, a couple of my colleagues have been working in the area of radiology and have shown that it's possible to improve the diagnostic accuracy of a typical, average radiologist by about 15 percent if you give him systematic principles for how he reads that *x* ray and how he records the data.

Another example: the issue of color in computer displays. There was a recent National Research Council workshop that was convened specifically to highlight the needed research with respect to color. They identified many unanswered questions about the way people react to color. Now color is obviously a compelling dimension, but we don't understand exactly what it is about color coding that makes it effective. If we have a hope of being able to make recommendations to the designers of practical equipment, those recommendations have to be based on principles that represent research that still needs to be done.

Current science funding supports, on the one hand, highly mission-oriented development, such as particularly in the military domain, and NSF emphasizes—NSF and some parts of NIH—emphasize investigator-initiated basic science, but there is a gap in between there where there isn't the initiative within the Federal Government that I am aware of for this area in between.

It is true that the Office of Naval Research, the Air Force Office of Scientific Research, the Army Research Institute, and the Army Human Engineering Laboratories in fact do support some of the kind of work that I'm speaking of. They are in the right domain, and you could think of that as being the kind of work, if you are familiar with any of the behavioral science work that they support, but their budgets are really disproportionately small in terms of both other areas of technology and the importance of the problems within the society at large.

Another kind of model is the NIH model. Aside from the grant program within NIH, there are contract mechanisms within NIH, in which NIH can identify a problem, and they can then go and actually stimulate the development of a research program in a particular area. That's a model that is quite appropriate, but my understanding is that with respect to behavioral area, that kind of model is applied almost exclusively in the medical instrumentation area and doesn't extend more broadly into other areas; and, further, that over the last 5 years NIH has moved away from the contracting mechanism and more toward the grant mechanism, which is fine for investigator-initiated things, but these are not the kinds of problems that investigators often initiate. They are the problems that grow out of a practical need.

So what I believe we need is some new initiatives, perhaps a new unit within an existing organization—I hesitate to suggest a new agency—but it should be a unit that is outside of the military. There are applications well outside the military that need to be supported. It would support research with respect to specific national needs. It would also identify and fund needed generic data and analytic tools.

Many years ago in 1960 the U.S. Public Health Service funded a study of human body sizes—height, weight, arm length, hand size, head circumference. It was a set of data that was extremely valuable to the Nation. That data is widely used in the clothing industry. It is used in workplace design. It is used in the layout of space vehicles. It is used virtually across the board in terms of the understanding of human physical size.

We are now at a state where we should be thinking about collecting data, more behaviorally oriented data, on a similar national basis: speed and accuracy of response to signals—highway signs, public signs, auditory warnings—a whole range of things which is a set of generic data. But what is a representative sample nationally with respect to such issues? Memory capacity, perhaps keying accuracy, since keyboards are becoming a part of our everyday existence. We need an organization which could deal with the collection and become a repository for data of this kind, and it would also have the initiative to identify needed design tools and models and fund their development.

In summary, then, I believe there is a critical national need for applied scientific research concerning the relationships between people and the equipment and systems that our technological society is producing. These needs are not limited to military systems. Rather, they span the range of Government and commercial applications throughout society. The products of this research have a direct impact on safety and productivity and touch virtually every individual in the Nation. When thinking about science policy and funding decisions, I ask you to remember this research need and to give it the resources and an organizational home that are commensurate with its importance to the future in our technologically oriented society.

Thank you.

[The prepared statement of Dr. Pew follows:]

TESTIMONY of

Richard W. Pew, PhD
Principal Scientist
BBN Laboratories Inc.
10 Moulton Street
Cambridge, Massachusetts 02238

before the
THE TASK FORCE ON SCIENCE POLICY
of the
HOUSE COMMITTEE ON SCIENCE AND TECHNOLOGY

on the subject of
The Role of Social and Behavioral Science

September 19, 1985

The Honorable Don Fuqua, Chairman
2319 Rayburn Building

Mr. Chairman, Members of the Task Force:

I am Richard W. Pew, Principal Scientist with BBN Laboratories Inc., a Subsidiary of Bolt Beranek and Newman Inc., a publicly-held company with a long history of research and development in the behavioral and social sciences. I have been in this position for eleven years. Prior to 1974 I was Professor of Psychology at the University of Michigan for eleven years. I have also served as president of the Human Factors Society and as first Chairperson of the National Research Council Committee on Human Factors. I am currently President of Division 21 of the American Psychological Association, the Division of Applied Experimental and Engineering Psychology. For 25 years I have been working in teaching, research, development, and consulting concerning applications of psychology to system design in both a university and an industrial laboratory. I am pleased to represent the more than 5000 professionals who identify with these fields and I bring with me a perspective that spans the range of settings in which this kind of work is being undertaken.

There has been a growing public awareness of the need for application of principles of behavioral science to system design, an area of utilization that is often overlooked in discussions of the need for research support. The field is variously called Ergonomics, Human Factors Engineering, Engineering Psychology, Cognitive Engineering or Applied Experimental Psychology. Although the need for research under these labels has been with us for a long time, in the last ten years it has received more intense public attention as a result of three kinds of developments: (1) Highly visible accidents like that at the Three-Mile

Island Nuclear Power Plant or the Bophal incident , (2) The growing realization that major deficiencies in military systems are a direct consequence of the inadequacy of current system design from the point of view of suitability for use and ease of maintenance, and (3) The growing public awareness that the effectiveness of introducing computers and other high-technology systems in the workplace, in schools and in the home is limited by the amount of attention that has been given to the needs and reactions of the human users of that technology. As computers and other advanced technology invade the domains of govenment agencies other than the Department of Defense, such as Social Security Administration, the Internal Revenue Service, and the National Oceanic and Atmospheric Administration, these agencies also are finding that human factors issues are paramount in their movement toward automation.

Let me speak first about the research needs in human factors. It is often said that human factors is nothing but common sense, but if it is, why is it not practiced more often? If we look around us, from kitchen stoves to automoble dashboards, it is easy to identify failures to practice what seems like common sense. Each of us could name a dozen instances of failures of design in our daily environments. I have been an expert witness in products liability cases involving needless accidents in the workplace; for example, a robotic inventory storage system in which a man was crushed to death by a computer-controlled cart that moved unpredictably, or a power press that permanently disfigured a woman's hand.

Common sense solutions look easy and obvious only after someone has found them. What you must do is place yourself in the position of the designer. He or she has to predict what will be the common sense solution before many thousands of copies are in the hands of consumers. He or she must predict what will be common sense for a wide range of potential users with different skills and experiences, not just for you. Finally, he or she must predict how a particular common-sense design solution will interact with a myriad of other design decisions all of which must be mutually consistent and compatible. It is because of this need to predict many users' behavior with respect to equipment that research is needed concerning human physical, physiological and cognitive characteristics, human preferences and human performance capacities and limitations.

In the book, "Bridge Over the River Kwai", Pierre Boulle projects the image of what is involved in the engineering of bridges. He says,

> "...It is essential to determine in advance the section and shape of every beam, the depth to which the piles are to be driven, and a mass of other details. Now each section, each shape, and the depth entail further calculations, based on figures representing the resistance of the various materials to be used and of the consistency of the ground. These figures, in their turn, depend on coefficients worked out according to 'standard patterns,' which in the civilized world are given in the form of mathematical tables. Mechanics, in fact, entail a complete *a priori* knowledge; and this mental creation, which precedes the material creation is not the least important of the many achievements of Western genius."

What Boulle is describing is the use of predictive models in bridge design. Such models are badly needed in the area of behavioral science. It is this ability to build predictive models of the human response to equipment that is required if we are to be able to practice the effective application of behavioral science to user-machine system design.

In the range of design issues to which I have been exposed during my career, I have found that most design decisions depend on characteristics of human performance for which data or predictive models either are inadequate, or do not exist. Thus, it is not just that research is needed in psychology. Rather, as in many other applications of psychology, the kinds of problems that need to be solved suggest particular kinds of research that are needed. On the one hand, it is not research that will arise spontaneously in an academic environment. On the other hand, specialists deeply involved in system development cannot afford to be distracted from their on-going work. The needed research is often identified in the system context, but needs to be championed and supported by other sources.

For example, there is great interest in the use of color in computer displays in government and industry. A National Research Council workshop identified many questions about human response to color that remain unanswered. Color is obviously compelling, but we do not understand what it is about color that makes it so good. Without that knowledge, we do not know when to recommend it and when not to.

Current science funding policy supports highly mission-oriented development on the one hand, such as that funded by the Department of Defense, and investigator-initated grants in the other hand, such as those funded by the National Science Foundation. Research is also needed that falls in between these two categories. This is research that is grounded in a detailed understanding of the applied problem, but that can lead to data and models that have more general applicability than the specific context in which the problem is initially defined.

It is this class of behavioral research that is most critical to the development of future technological systems, both in the military and in the commercial sector, and it is not receiving the funding that is needed. The Defense Advanced Research Projects Agency currently supports almost no behavioral science research. The Office of Naval Research, the Air Force Office of Scientific Research and the Army Research Institute support some behavioral research at the right level of specificity, but their budgets for behavioral science are disproportionately small, both in relation to other areas of technology and in relation to the importance of the problems being addressed.. Further, there is a strong need for this kind of work outside the military establishment.

The National Science Foundation does an admirable job of supporting investigator-initiated fundamental research and the Behavioral and Neural Sciences Division as well as the Information Science and Technology Division encourage the submission of behavioral studies. However, there is no mechanism within NSF for initiating research at the intermediate level I have described that I believe is critical and seriously underfunded.

The National Institutes of Health provide another model for funding. There are units within NIH that focus on particular research areas and that have both contract and grant authority that can take initiative with respect to research in specific areas of critical need. However, the only work on human performance in relation to systems that falls within the NIH charter concerns medical instrumentation. Further, this agency has been moving away from the much-needed contracting mechanism in favor of their grants program.

A new unit or agency outside the military context is needed that is specifically chartered to support applied research on the relationship between people and equipment and that has the NIH model of funding authority. It could be a unit within NSF or elsewhere. In addition to stimulating research on specific problems of national need, it would identify generic data and analytic tools that are needed and would initiate projects to obtain them.

Let me illustrate one example of such generic data. National norms were obtained many years ago by the Public Health Service on human body sizes, including overall height, weight, arm length, and so forth; there is now a need for comparable nationally representative data on fundamentals of human performance. The needed data might include the speed of response to signals, memory capacity, or the speed and accuracy of performance with which simple devices such as the telephone keyboard are used.

This unit or agency would become the national repository for such data and would obtain and systematize existing data as well as initiate the collection of new data. I could imagine such a unit being organized within the Bureau of Standards.

In summary, there is a critical national need for applied scientific research concerning the relationships between people and the equipment and systems that our technological society is producing. The needs are not limited to military systems. Rather, they span the range of government and commercial applications throughout the society. The products of this research have a direct impact on safety and productivity and touch virtually every individual in the nation. When thinking about science policy and funding decisions, I ask you to remember this research need and give it the resources and organizational home that are commensurate with its importance to the future of our technologically-oriented society.

DISCUSSION

Mr. WALGREN. Well, thank you very much, Dr. Pew. That's something we ought to think about and try to understand more deeply. I am struck by the new brake lights on cars that are now brought up into the back windshield. If there is a documentable benefit to that, we went quite a long time before we realized it, and I'm sure if there is a documentable benefit to that—and I assume there is, in response time and what not—have suffered a lot of damage for failure to do that in 1935, I gather.

Dr. PEW. The Department of Transportation funded a study, several studies—the one I am familiar with was with New York taxi drivers—in which they actually placed such lights in half of a sample of taxicabs and not in the other half, and they then had longitudinal data on the number of rear-end accidents, and it has a very significant impact. My recollection is a 40-percent reduction in rear-end accidents.

Mr. WALGREN. Perhaps the program that you suggest sounds a little bit like that program in the NSF in the early seventies, at least "Research as to Applied National Needs," or something like that——

Dr. PEW. RANN? Yes.

Mr. WALGREN [continuing]. And yet that was not encouragingly received by, I guess, judgments made at the time.

Dr. PEW. That's correct. I wasn't a very direct party to that but my understanding was that the focus of that program turned much more strongly toward the social sciences and away from the kinds of areas that are represented here. I may be mistaken about that. I'm not certain, sir.

Mr. WALGREN. I see. Well, I wanted to ask generally how you folks would assess or estimate the benefit of the next 15 years of social and behavioral science research that we would be trying to secure if we were to have what you might call solid funding or at least consistent support. I guess in this area you always look for immediate benefit. One of the earlier witnesses was talking about the principle that human beings will pursue rather direct benefit, economic utility or whatever it is. I have always had the inkling that there was a lot of economic utility in this whole area that was at least underappreciated, perhaps because it hasn't been realized, but nonetheless that there is real direct value to what might be made real to our society by the social and the behavioral sciences.

It seems one of the underpinnings, although I don't know whether this is true, but I also feel that the public got a good demonstration of the utility of the physical sciences with the atom bomb. I mean, nothing focuses your attention more than a potential hanging, and I think that the consistency and the unquestioned support of the pursuit of the physical sciences following that very dramatic lesson their impact has not been lost on the public, and has translated into substantial support.

The behavioral sciences have not had the same advantage in many ways, and yet could you or would you offer a comment on the judgment that the social sciences—with respect to advances in near term, yet to be made, that would come out of research, both basic and applied, in social sciences—would have a very large beneficial contribution to make to the American economy and to the well-being of American life? Is that something that you would like to comment on?

Dr. ETZIONI. I am reluctant to be the first, but that will allow my colleagues to correct me.

I don't have any doubt that social science can and will make a major contribution. In fact, I liked the phrase used by Dr. Sutton earlier when he talked about "inevitability" of the social sciences, because what I tend to focus on is not only on the specific study, to look for the one specific experiment with the one specific finding which turns a program around. There is another major way in which social science is continually used and ought to be used, and that is completely changing the way we think about things.

For instance, to just give quickly a few examples, before social sciences took off, we used to think about children as if they were small adults, and we used to expect from them that they were fully willful whenever they misbehaved. We learned over the years that they have to first be developed. We all share that perspective now, but its roots are strictly in the social sciences, and in effect we have not completed the transition. If you studied the last reports on the various commissions on education, many of them are rather short on social science, and once their findings would be put through social science filters I think we will see how differently we will have to approach schools than simply pumping more academics into them. It probably has to do with self-discipline and other such things, as for instance the Coleman data shows very nicely.

Just one quick other example. These days we have a new movement. It's not that new any more. We tried to Japanize our factories, so a changing of corporate culture, but many of my friends in the corporate world still think that corporate culture is something you can order together with your raw material and your paper clips. You can change it. In fact, a very major American corporation ordered its executives to be more participatory, and you can imagine the kind of chuckle that produced at the other end of the line. Now some of us studied for a long time and we have fairly hard evidence of what it takes to change a corporate culture.

In effect, you can close your eyes and think about any public program, and you will see that behind it, if it is dealing with criminals or with alcoholics or with mental patients, are social science assumptions and social science findings. Let me just cite one rather dramatic example. A few years ago some of us approached the U.S. Air Force with the following question. The fail-safe you mentioned, Mr. Chairman, the nuclear problem, in which we are supposed to recall the bombers if necessary or they are supposed to return at a certain point, is based on the assumption that if you send a communication through several layers of command it will be understood at the other end.

We have a lot of evidence to suggest that it ain't so. Just to quickly remind you what that feels like, children play a game—I think it's called "broken telephone"—where they sit all like this and they transfer a sentence down the line, and by the time it comes six children down it is always garbled. Well, that is exactly what our studies support, and they show that you have to concern yourself whenever you send a communication through several stations that it be properly received at the other end. At that time there was no provision for correction and verification. It was introduced after the suggestion of social science.

So, in short, if you take into account not only specific findings such as the one Dr. Rees mentioned about the fact that every 5 years, I think, 100 percent of the poor turnover or 20 percent a year, which is a very different view of the poor, as there are poor people and there are nonpoor, and they are a separate breed of people. Aside from the specific findings, we are very deeply ingrained in affecting, as you ought to, the way that the secular world approaches all of our social, economic, and interpersonal problems.

Now one last footnote about the RANN question, and it deserves more study and consideration. I in no way suggest that I exhaust that now, but just one footnote. We like to deliver footnotes. That was putting a policy-oriented unit into a basic-oriented agency, and that is really somewhat like asking the fox to attend to the chickens. I would think that with all due respect, again, if it would be up to me I would increase the NSF budget by a factor of 10 just to start with. OK? So I am all in favor of NSF, but I really wouldn't necessarily always tend to NSF to develop a policy-oriented mission.

Dr. **Rees**. Mr. Chairman, could I take a crack at your question?

I am going to approach this in a rather different way. I think it is very, very hard to promise in what areas we are going to make progress. I think what I would like to suggest is, there are some very difficult problems that we don't understand, where I think with a little bit of effort we could probably improve our understanding. Even though we can't guarantee that we will make progress, the problems are so pressing that they deserve the effort.

Let me just cite one. I have been writing an article over the past few months, reviewing the literature on youth unemployment, and I submit that that's an area where there is a major puzzle that nothing I have seen in the literature comes close to solving. Over the last 30 years the employment-to-population ratio of black teenagers has been going down, down, down, down. The employment-to-population ratio of white teenagers has been almost exactly constant. If anything it has been rising a little bit. There is this enormous difference in behavior, participation in the labor force, between black teenagers and white teenagers. I don't know whether the social sciences can find the reason for that, but I suggest it is so urgent that we do find the reason for it, that we ought to fund the effort.

Mr. **Walgren**. Dr. Pew.

Dr. **Pew**. I guess I would like to respond in two ways, Mr. Chairman. The first is, some years ago I heard a lecture by Lester Thurlow, the economist, in which he talked about the productivity problem as "death by a thousand cuts." That is, the productivity problem is not any one problem, but it's a whole collection of factors which together lead to the fact that we have had low productivity.

I think the same thing may be true with respect to the behavioral and social sciences, that we shouldn't expect to find a single breakthrough or to predict a single breakthrough over the next 15 years, but rather that there is the potential for continued gains across the board. While we may not be able to predict exactly the kind of place in which those are going to occur, they are going to occur on broad fronts as a result of funding of this kind of research.

Within my own area I think one of the developments that I see on the horizon is beginning to make the engineering design process more systematic in the sense that it begins to call on the use of designed work stations, computer-aided design, computer-aided engineering stations. The implementation of such stations provides an opportunity to bring into the perspective of the design process, user information, the kind of data I am advocating be collected, in a way that has never been easily possible before. It is easy for an engineer to ignore a handbook when he doesn't feel he understands it. It is much more difficult for him to ignore it when it faces him in the context of a design work station where these are factors that he must consider in order to proceed with his design. So I think I see some leverage being made available for the introduction of this kind of information in the design process in a way that it hasn't been possible before.

Dr. SUTTON. I suppose we all have examples and reflections from our different perspectives and points of view. The general remark about your question, sir, is one that links back to something I was saying earlier. I think success always has many parents, and it is typically very difficult in all sorts of practical achievements, even in the application of the natural sciences, to determine the role of the scientific element in the undertaking. A great lot of what the social sciences do for society is in the avoidance of folly and losses, and just if you consider, for example, that if we had perhaps had better economics before the Great Depression, we might not have had such a severe depression, and we all, I believe, know that the Great Depression cost us more than the Second World War. The prospect of avoiding a similar national catastrophe is something that is one of the important issues always before us. If we avoid it, we will say, "Well, that was the way it was, and aren't we clever? Aren't all of us rather clever?" To sort out the role of the contribution of economists and others in the management of our economy to avoid that kind of national catastrophe is often difficult to detect.

I can think of—in the international sphere that interests me particularly—I can think of very notable successes in the last years in which the social sciences have certainly played very important roles, but other things have, too. We are all now preoccupied with the prospect of African famine. We were preoccupied in the sixties with the prospect of Indian famine. We are delighted now at what is really a great human achievement, the effective resolution of the food production problem in India. Now this has a complicated history, but certainly the social sciences played a very important role in guiding the kinds of policies that took the kind of political leadership that is indispensible to any kind of national achievement, and it enabled them to do things they otherwise would not have done.

Similarly, it is often said that the relative success of our policies vis-a-vis China has depended upon the fact that we have built up a cadre of people in this country who are real professionals in the understanding of China, and that in some other parts of the world where we have done less well, we had less good people to analyze those situations, so that many of the things that we see as great human achievements in this country and elsewhere have an element of social sciences in them which is often difficult to detect, and perhaps we need a little more careful attention to celebrating those things.

Mr. WALGREN. Along the same line, if you were responsible for a range of science budgets, and you look at the National Science Foundation, for example, with a very minimal investment in social and behavioral sciences compared to the other range of efforts that they make—and I understand it's something like 4 percent or thereabouts of their total effort—would that be the kind of balance that you would strike if you were trying to maximize the social utility of rational research?

Dr. REES. I think I would increase, if it were up to me, the budget for behavioral and social science in NSF, but I wouldn't do it suddenly. I would do it gradually, and I don't have any notion what a proper predetermined figure would be. The natural sciences are much, much more expensive inherently than the social sciences. A particle accelerator is just a horrendously expensive machine. I don't know of any way that the social sciences could productively use the kind of money that goes into high energy physics. It's just a whole different kind of ball game, so I think it's inherent in the situation that social and behavioral science be a small fraction of the NSF budget. It is inherent to the needs of the disciplines, just as within the natural sciences mathematics is much less expensive than physics, because most mathematicians need a pencil and paper or a computer. They don't need experimental equipment.

But, having said that, I do think that the Federal funding of the social and behavioral sciences was cut in real terms starting in the early 1970's. It was cut sharply in 1981, I think as a result of animus against the social and behavioral sciences by some staff members of the Office of Management and Budget, and while a good deal has been done to restoring those cuts, they have not been fully restored. They have certainly not been fully restored in real terms, so that maybe as an interim goal what you should say is not to aim at some particular fraction of the NSF budget but to aim at getting the support for social and behavioral science back in real terms to where it was in the 1970's.

Mr. WALGREN. You mentioned there that we ought to be guided by a sort of an assessment of need in the individual areas, and that more than—that that's a better judgment than relative spending would be. Are we in a position to make a good assessment of need in various social and behavioral science disciplines. Is that something that is being done by somebody in a good way?

Dr. REES. Well, I assume that the people in charge of the directorates in NSF have some method of deciding what the needs are of the different disciplines within their directorate, and also the areas that seem most promising and the areas that seem not to be making such rapid progress or not to be submitting such good proposals. You have indicators like, you know, the fraction of proposals that you receive in a field that you are able to fund, the fraction of the ones that get high ratings that you are able to fund. I think it has got to be, like most budgets, more of a bottom-up process than a top-down process.

Mr. **Walgren**. I see. Any other comments?

Dr. **Pew**. Again, it seems to me that that is certainly one of the processes that needs to be done, and the suggestion that one looks at what is being submitted and then takes that as an indication of where the interest is with respect to science, I really think that there is a need for more of an advocacy view. That is, there is a need for the agencies within the Government to identify those areas that are particularly promising and to actually provide the funding that will help to bring those developments about.

I have been a member of a National Research Council Committee on Human Factors where our major goal has been to try to identify research needs within the domain of human factors. That kind of effort provides an agency with a kind of a view of what the potential areas for support are.

The most effective bureaucrat that I ever worked with was a fellow that worked with NASA, who came into the organization completely naive, and he knew very little about the area that he was responsible for. He spent about 6 months traveling around the country, talking to everyone that he could find, finding all those people identified as experts, and on the basis of having accomplished that information-gathering effort, he then set about and decided for himself what he thought were the most promising areas and where the funding should be put. He not only put the funding there, but he went out and actually served as an activist to encourage submissions and to encourage developments within those particular areas. That kind of an activist advocacy view is one that I think can be very effective in these domains.

Mr. **Walgren**. Dr. Etzioni.

Dr. **Etzioni**. I would like to continue along the same lines. While on the one hand I believe much of it will have to come from supporting basic research, the question I would like to talk for a moment about—beyond what we said earlier—is how, where there are Federal needs, programmatic needs, if they articulate more clearly to the social sciences, how the social sciences could then respond to them, and as a result the two would get together more quickly. I'll give one example from the GAO and one from the Inspectors General.

When the GAO, which is after all an arm of Congress in a sense, studies various social programs they apply to them the skills of accountants or people who have been trained in the FBI schools of investigators, and I think both of these things need to be applied because often the question is of money spent unduly, et cetera. The answers, though, that tends to bring to is the question of appropriateness of the paperwork transactions and such, then maybe go beyond that to do what we would call system analysis. That is more near at home for applying social science to understand, for instance, why community mental health centers do or do not work, and very often the social sciences are not participating in GAO in studying social programs, but instead they rely on people whose training is very far from such analysis.

It's even more extreme in the 12 inspectors general we set up. I happen to have had some firsthand insight into that. The year I spent working for the Government, I visited with some of these new units of important Federal agencies. Again, if you would do a profile of the skills of the people who do system analysis to see if we can prevent fraud and abuse before it occurs, rather than try to catch people after it occurs, you see these are the kinds of things social science could be quite helpful in analyzing, and you will be surprised how few people with any kind of social science training are in the 12 inspectors general bureaucracies, and some of them are quite sizeable. They again come from a similar background to the people who work for the GAO.

Now many more examples could be given, but it illustrates the point that sciences in the end respond not only to their inner dynamics but also to the pressure of needs and questions we need to resolve, to the kind of example that was given about black unemployment. So if the agencies entrusted with studying domestic social programs would build bridges to the social sciences, I think the social science yield on the application side would rise more rapidly.

Mr. WALGREN. In the Federal Government we often wish that other parts of the society would fill certain roles which we don't think it is appropriate for us to fill, and you indicated, I think, Dr. Rees, that the foundations would instinctively think it somewhat proper for them to be funding controversial social science research, as opposed to the Federal Government, anyway. I'm sure from the Federal Government standpoint we would like that to be the case, but what I hear from both you as foundation representatives is that you can't count on the foundations in this picture because you have really substantially retrenched your commitments, at least as to the Ford Foundation, and I guess I don't know what—your foundation, Dr. Rees, brings a relatively focused—you have selected a couple of areas that are quite finite and not broad ranging, anyway, across the frame of social sciences.

So would you encourage the Federal Government to believe that some other sector in our society would be there to do some of the needed research that we might not instinctively be ready or able to fund as a Government, and yet clearly we would wish would be carried out? For example, the thought of losing the longitudinal income study is really a scary thought when you think about it. It's almost as scary as the thought of what the research results might be in the first place for all of us. That we too might slip down is a real jolt to our system. But would you encourage the Federal Government to take the approach that there is an area here that should be left to the private sector, left to the foundation sector?

Dr. REES. Well, the Sloan Foundation's grants in nominal dollars have been going up. There hasn't been the kind of decline that the Ford Foundation experienced, but they have not been going up in real dollars. They have probably been going down in real dollars. In other words, the grants in nominal dollars have not kept up with the rate of inflation.

I don't see on the horizon any new foundation that's coming along that's likely to be a major funder of the social and behavioral sciences. The largest private philanthropy in the world will, inside of a few months, be the Howard Hughes Medical Institute, but that is going to be strictly devoted to basic medical research, so I don't see any hope for the social and behavioral sciences there. The other new foundations that have been formed recently, like the Keck Foundation, don't seem so far as I know to have an interest in supporting social science research, so I think, so far as the private foundations are concerned, what you see is what you'll get. Any support that is forthcoming is likely to come from the foundations that have already been doing it on more or less the same scale that has been experienced in the recent past. That permits a certain amount of filling of very urgent gaps, but not very much.

So far as corporate philanthropy is concerned, I meet regularly with a lot of corporate foundation people. I can't think of a single case in which a corporate foundation or a corporate giving program has made very substantial grants in the area of the social or behavioral sciences. That's not a high priority. They are much more likely to make grants in the arts and the humanities, particularly the arts, than they are in the social sciences, but basically the focus of corporate philanthropy seems to me to be in social service. That is highly desirable but it doesn't solve the particular problem that this task force is trying to deal with.

Dr. **Sutton**. Perhaps on a little more optimistic note, I do think that there are subjects from time to time where there becomes a particular interest in the private foundations. That can be very complementary to what is supported by the Federal Government or other public agencies. There are periods in which a foundation picks up a subject and gives it more attention than is likely to be the case from the Federal Government or otherwise.

Right now, for example, the Ford Foundation is supporting a great deal of work on migrants and refugees. That has been, for the last few years, a very considerable interest of the Ford Foundation, and I think that it is obvious that this is not just—some of this is not especially controversial, but it happens to be a particular interest, and I think that a great contribution is, I hope, being made to the understanding of what it means to this country to have this very considerable influx of migrants and refugees that we have had in recent years, with problems of adjustment and so forth.

Now that kind of thing, obviously, needs to be seen as complementary to what is being done by the Federal Government and other public sources, and that will happen from time to time. There was a time—I mentioned China a bit ago—there was a time when the Ford Foundation provided about 75 percent of all the research moneys that went into the China field. We don't do that any more. It conceivably could recur, but there has to be a kind of a complementarity, it seems to me, between the private and public sources in this sort of venture.

But overall I think that what is worrying is the decline of scale of a lot of things, this problem of keeping up with the real support of things. I quoted in my testimony this "Beyond Growth" study that was recently done under the auspices of the Association of American Universities. There it contains the rather pathetic suggestion that the decline of funding for a lot of international studies has led people to lower their sights, so that lots of things that ought to be tried and done, people now don't even venture to put up proposals for them because they are discouraged at the prospects of getting anywhere with them, with the result that particularly projects that involve several investigators and that need several years to run have very limited sources of funding in a number of fields now. It is an argument, incidentally, for a substantial increase in budgets for the social sciences in a way that perhaps complements what Dr. Rees was saying.

Mr. WALGREN. Your thought of hoping that what the Ford Foundation is doing with respect to migrants would be appreciated or understood and certainly comprehended by the Federal Government brings me to a question that Chairman Fuqua wanted to ask but had to leave. It has to do with the ability to deal with these multidisciplinary—or I guess problems that are so general, they span a number of agency interests, specific interests. It is said to be understood that the new research areas in social and behavioral sciences appear to be multidisciplinary.

Would you like to comment on the difficulties, the fact that Federal agencies are not really organized to span the breadth of the concerns, but rather are organized to reflect traditional academic disciplines—or at least much of our Federal establishment I understand is organized to reflect traditional academic disciplines—and the question is, does the present structure of these agencies, and I think particularly the NSF which is relatively vertically structured, inhibit a multidisciplinary approach or an ability to appreciate some of the results that might be developed when you study a problem from a number of different ways? Do we have organizational problems at present in the system that have to be broken dramatically?

Dr. REES. I think the answer to that question is yes, we do, and a perfect example is the one that I mentioned in my opening statement about cognitive science. Cognitive science is an emerging interdisciplinary area that draws on psychology, neuroscience, artificial intelligence, to some degree on linguistics, on philosophy, even on anthropology. The two private foundations that have funded centers for cognitive science—incidentially, one of your witnesses, Herb Simon, is one of the founders of that field—the two foundations that started funding this interdisciplinary effort both set up interdisciplinary centers within major research universities. Now the foundation funding is running out. The foundations will turn to other things. It is proving hard to find a home for that within the Federal funding agencies because it doesn't fit neatly into any of these disciplinary boxes.

There are two kinds of solutions for that. One is to create new units, so that you could create a unit within NSF or NIH called Cognitive Science and fund it, but the other solution would be to put a certain part of that budget for expenditure at the higher levels of NSF with the specific understanding that it was not to be doled out to particular disciplines but was to be used to fund interdisciplinary efforts. Now the objection that will be made to that is, "How do you run a traditional peer review system in connection with an interdisciplinary effort?" I suppose you would have to have some sort of an interdisciplinary peer review process. That would be difficult to do but I don't think the difficulties are necessarily insurmountable.

Mr. WALGREN. Dr. Etzioni.

Dr. ETZIONI. I very much agree with the point already made that part of this could be handled inside the NSF, whose ultimate mission is and will continue to be the basic sciences, but equally I would emphasize the need for—I would favor more hearings and even digging in what happens in the mission-oriented agencies because obviously they, on the face of it, are in the multidisciplinary business.

Just to give one example out of many hundreds which deserve some more study, when we had the energy crisis, obviously, the problem of energy was created, and one of the major issues was how to encourage conservation. Now in part this is obviously a behavioral question—under what conditions people change their consumption habits, driving their cars as against walking or riding their horses or using the bus, turning down their thermostats. All these are behavioral issues, but the Department of Energy did not have great enthusiasm for going into the behavioral sciences.

A blue ribbon commission was created—this was in the early eighties—to study the question of the research program of the energy program. One of its major recommendations was to increase the social science component of the energy research program. I don't want to start here dealing with gossip. I think a systematic study should be done because it would be very illuminating. How did it happen that, after a blue ribbon commission of national science experts unanimously recommended that the social science component will be increased, that it was not done?

Now I am not arguing here about the budget for my part of the woods. I think any straight-thinking person will realize that there is a major behavioral component to this issue. Again, it's not a question. I singled out the Department of Energy because it gives you a convenient example. I think you could find examples like this in most mission-oriented agencies, and before we solve the problem you have very correctly identified, I think we will have to understand better what it is which, when the issue is multidisciplinary, we often get a group of scientists from one background—physics is your best nominee—who then secure that that gets the deserved attention, and the other sciences are not brought in.

Last, just to reiterate, we need to find ways to train more people who are able to do it because otherwise we have the problem that, even if one of the mission-oriented agencies tomorrow suddenly turned its full enthusiasm—that is somewhat difficult for me to imagine—to really do multidisciplinary work, they will not have the people with the training ready because that requires one more step. It is not simply a question like we are short of math teachers, then we'll train them in math. We often don't have developed the body of knowledge we need to give to them, so that requires even a little more preparation.

There the question is if we should not—we did that once in the education area—create two or three or four centers which go into the preparation of the body of knowledge necessary to facilitate multidisciplinary research. But the main question is—if you will forgive me for that term—on the demand side. If the demand will be more articulated, I think the supply will eventually correct itself.

Mr. WALGREN. Dr. Pew.

Dr. PEW. The substance of some of my comments actually was directed toward the issue of the multidisciplinary problem. To pick specifically the NSF—and with respect to my limited knowledge about it, I don't claim to be an expert on how NSF is organized—but I think when you are trying to fund interdisciplinary research, it isn't as successful to simply bring a representative of the disciplines together and say, "This committee is going to administer work in this interdisciplinary area," because each of those individuals winds up having his own priority and his own agenda, and it doesn't blend. It is much more important that you establish a unit and have the individuals staffing that unit have that interdisciplinary interest and background already.

Now they obviously aren't experts in all of the disciplines, but they bring to it a perspective and a knowledge of how the disciplines interact. The point about peer review is an excellent point. A big problem in interdisciplinary research is that when it gets sent out to individuals representing the various disciplines, no one discipline sees it as important, when in fact it is the aggregate of the groups to whom it is important and not to any one of the individual disciplines. So the peer review process has big problems in terms of being able to understand the real contribution of a piece of interdisciplinary research.

Mr. WALGREN. Who can best represent the aggregate at that point?

Dr. PEW. I say it's an individual who undoubtedly has his original training in one of the disciplines, but it's an individual who has been working across these disciplines and has seen the interaction and seen what the potential contribution in the aggregate is.

Mr. WALGREN. And those individuals should be findable if the thing has ripened to the degree that we're considering?

Dr. PEW. Yes, I think that's true.

Mr. WALGREN. I had suggested liberal arts graduates to the NSF one time, and it didn't really compute, I don't think.

Well, let me ask one other question and that is, if you had to choose one thing that we should try to do in the relatively immediate future to strengthen the health of the social and behavioral sciences, what would instinctively that be in your mind? It's the old "one wish" from the children's story.

Dr. SUTTON. I don't want to give you a very specific thing. I just wanted to emphasize once again that the support to the basic social sciences seems to me not adequate to sustain the kind of adaptation to all these interdisciplinary demands, to the changing boundaries of fields, to the complexity of the problems as they present themselves, and that seems to me only to be managed in a hopeful way if we have an increase in funding for the basic social sciences. I am impressed, as I take on the responsibilities for the Social Science Research Council—the Social Science Research Council, I am happy to say, is flourishing in most respects—but when it comes to trying to find funding for things that really relate to the basic social sciences, one of the problems, the responsibilities in an interdisciplinary way of the Social Science Research Council, that is the most difficult kind of thing to get funding for, either from public or private sources, that the Social Science Research Council faces. We are doing very well, thank you, on a lot of fields but not on basic social science.

Mr. WALGREN. Thank you.

Dr. Rees.

Dr. REES. I guess I would come back to my one wish that there was a little bit more money for the collection of data bases. To give a specific example, Tom Juster, who is the director of the Institute for Social Research at the University of Michigan, has been trying very hard to put together funding for a study of the use of time. This is something—it's an interview, a survey research project—that has been done a couple of times before but not for several years, and we need at least a third point in that data set. It's an interdisciplinary thing because it would be used heavily both by economists and by sociologists, and possibly by others as well. The amount of money involved is something like $2 million.

Last year he did something very unusual, in fact something we have just said is almost never done. He got support for that project from both the Economics and Sociology Panels in the National Science Foundation, but the NSF funding wasn't enough to do the whole job. He also needed some private foundation funding and some NIH funding, and the NIH funding fell through, so now he is starting all over again.

But I wish there were some funds not only for the ongoing support of longstanding data bases like the panel study on income dynamics, but for occasionally collecting a new data base which will be useful to the social sciences. That, the one I have just mentioned, I think is a perfect example of that need.

Dr. ETZIONI. My one wish would be that you unlock the doors which are between the mission-oriented agencies and the multidisciplinary research. I asked once the head of OTA and he explained to me that not only technologies but techniques were also part of his domain, so it may be appropriate if a congressional committee could charge OTA with studying the question of what it is—the kind of question you formulated a few minutes ago—what it is we need to advance multidisciplinary research, not out there in academe but in the interface between researchers and the mission-oriented agencies. I would see if there is a way of getting GAO to look inside and evaluate its own evaluators exactly from the same viewpoint, and there must be other avenues to understand better why we don't get more yield from what we have, aside from needing to increase the social science bases.

Mr. WALGREN. Dr. Pew.

Dr. PEW. This is probably the most difficult question that you have asked, but I think my wish would be that, in addition to maintaining or increasing the sort of basic science funding, that there would be a few areas, perhaps some of them interdisciplinary, that would be targeted over a 5-year basis or targeted over a period of time, where within that area there would actually be some advocacy for building up a research base or building up a capability in particular areas that look promising or have the potential for big payoffs. In a few places where I have seen this happen over the last several years, I think it has substantially enhanced our capability nationally to deal with the class of problem that was presented. I have the feeling that rather than just sort of sprinkling money across a broad area, particularly in these interdisciplinary areas, it would be better to focus them and to identify some priorities for focusing. I haven't told you how to identify those priorities.

Mr. **Walgren**. OK. Well, let me, on behalf of those involved in the task force, thank you very much for your effort that you made to make these comments this morning. We appreciate both the testimony, and we also appreciate your being a resource to us in the past and, we hope, in the future.

With that, we will conclude these sessions on the social and behavioral sciences, and I don't know what the next session of the task force is, but we appreciate your effort in being here, and we look forward to talking to you about these things in the future.

Thank you.

[Whereupon, at 12:10 p.m, the task force recessed, to reconvene on Wednesday, October 2, 1985, at 9:30 a.m.]

[Questions and answers for the record follow:]

Task Force on Science Policy Hearing
on
The Role of the Social and Behavioral Sciences

September 19, 1985

Questions for the Record

Dr. Rees

1. **Certain criteria may apply generally to the Federal Government's support of science. In making decisions about the support of the Social and Behavioral Sciences by the Federal Government, what criteria should apply?**

   Progress in the social and behavioral sciences is heavily dependent on the continuity and quality of large-scale data bases funded by the Federal Government. Maintenance and improvement of these data bases should be given high priority.

   In the funding of basic research in the social and behavioral sciences, the Government has been well served by the peer review processes established by the National Science Foundation and the National Institutes of Health, which should be continued and protected. At times, special funding is needed for emerging interdisciplinary fields in which peer review processes are not yet established. Such needs can be met by directing a portion of the support of NIH and NSF to special funds for emerging fields.

   Mission agencies such as the United States Department of Labor should have funding for the support of applied social science research that is relevant to their missions.

2. **It is widely recognized that many of the changes now occurring in society are attributable to the advances taking place in the physical and life sciences. To what extent do the social sciences help the Nation make informed use of the discoveries and technologies produced by the physical and life sciences?**

   The use of social science research to help the Nation apply discoveries from the physical sciences is of great importance. For example, the biological sciences have demonstrated beyond reasonable doubt that the use of tobacco is seriously injurious to health. To translate this finding into reduced consumption of tobacco, especially by the young, requires insights from psychology (as applied, for example, to warnings) and from economics (as applied to excise taxation). Many similar examples could be given in such fields as automobile and aviation safety, which involve complex interactions between technological systems and human behavior.

3. **The deeper understanding of human needs and social processes which is expected to be achieved from research in the social sciences should provide an enhanced level of understanding relevant to national policymaking and priority setting in many areas. What contributions to national priority setting should the social sciences be making that they are not making now?**

Despite decades of substantial expenditures on anti-poverty programs, the United States still has intractable problems of hunger, homelessness, and teenage unemployment. I believe that the social sciences can contribute both to improved understanding of the causes of these problems and to improved programs to alleviate them. Such improvements will not come quickly or easily, but might come sooner if there were more support for research on these issues.

4. **Research in the social sciences has advanced steadily in the decades since 1945, and many findings have emerged. The use or nonuse of these findings is not, however, automatic, and may or may not be justified in particular circumstances. What is the role of the Government in facilitating or inhibiting the contributions of the social sciences to the resolution of issues of national importance?**

Many leading U.S. universities now have schools of public affairs or public policy which are in effect schools of applied social science. They stand, vis-a-vis the social sciences where engineering schools stand vis-a-vis the physical sciences. Most of the graduates of public policy schools begin their careers in State, local, or Federal Government, and serve as a principal transmission belt for applying social science knowledge to issues of national importance. A similar function is served at a higher level by the appointment of senior social scientists to temporary positions in Government service. Such appointments are relatively frequent in economics, but much less frequent in most of the other social sciences

5. **In making judgments about the budgetary levels of support for the social sciences, the Congress has studied the proposed budgets for the individual disciplines, such as anthropology, economics, and psychology. But it has customarily authorized and appropriated funding in the aggregate, encompassing all of these disciplines in one lump sum. The diversity of the individual social science disciplines and their differing degrees of maturity and relevance to societal problem solving might suggest that the Congress differentiate between the levels of support to be provided. On the other hand, the ability to make such judgments, and the time required to delve into the state and opportunities in each field, might mitigate against such an approach. To what extent should Congressional support distinguish among the individual disciplines within the field of social and behavioral sciences?**

In my judgment, it would be a mistake for the Congress to involve itself too deeply in the questions of how much funding to devote to each of the social science disciplines. Such judgments are better made by the National Science Board and by the directors of the National Science Foundation and the National Institutes of Health, who are closer to the scientific work being done in these disciplines. The principal concern of the Congress should be with the level of support for the social and behavioral sciences as a whole.

BLANK PAGE

266

APPENDIX

April 25,1985

Don Fuqua
Chair, Committee on Science and Technology
U.S. House of Representatives
2321 Rayburn House Building
Washington, D.C. 20515

REFERENCE: Report of the Task Force
On Science and Technology

Dear Mr. Fuqua:

A copy of the December,1984 Report of the Task Force on Science and Technology has reached my desk. As a scientist and as a citizen with a long standing interest in the precise problems discussed in the Report, I was most pleased to see that the Task Force was asking these questions.

This letter has three purposes: First, to request that I be able to appear at public hearings, when these may be scheduled; Second, to comment particularly on Section IX Funding Mechanisms; Third, to offer a solution to the problem posed in Section X.C.2, "How Can the Congress Make Independent Judgements about the Quality of Past, Present and Future Research?"

1. Request to Appear at Hearing.

My request is to appear as an interested citizen or individual, not as the representative for or employee of any institution. My opinions are my own.

However, certain background details may establish my interest in this proceeding. A recent resume is included with the letter. In summary, I have credentials in several fields of science, and employment experience in equally many fields both in and outside of the academic world. I have performed and published original research in several fields, both while in the academic world and since that employment. I have regularly given technical lectures or published scientific papers in fields both related and unrelated to my current means of earning income, throughout my personal history.

I have maintained a long standing interest in the precise topics of your Task Force. I have been both funded and not funded from various federal science programs. I have been active in addressing detailed analysis of these programs to members of Congress since at least 1975. In the late 1970's period I edited and published an editorial newsletter spercifically directed to present questions and circulated this, entirely at my expense, to members of Congress and others. I supported action by the Texas legislature to eliminate academic tenure at state institutions and to replace it by the present system of well defined institution and faculty rights. I was an active supporter of the Small Business Innovation Act which affected science funding and distribution rights.

The views I thus represent in this letter and would communicate personally to your Task Force are those which I have long and consistently held and advocated, and which are based on a depth of experience which has only strengthened my conviction on these matters.

2. Funding Mechanisms.

The relation between individual scientists, the institutions to which current law and practice drives them, the quality of resulting science, and essential questions of individual rights and perhaps even Consitutional rights, are quite complex, as reflected by the breadth of your own Task Force Report. Rather than review the failures of the current process (of which in any event your Task Force seems well aware) I shall simply list key reforms.

--Named Individual Responsibility.

A unique feature of present science funding is the absence of individual responsibility for decisions. While bureaucracies commonly avoid responsibility by distributing it, the science agencies are unique to our system of legal responsibility in permitting completely annonymous comments by "peer" reviews, and subsequent masking of decision criteria by agency officers. The commonly stated "need" for or justifications of this annonymity simply is not correct. Scientific reviewers are in no sense "disinterested" when it comes to areas of science about which they have knowledge. Such reviewers are directly interested in that they, or entities with which they have financially beneficial interests, are very certainly supplicants for the same funds over which the reviewer has influence.

Even were the monetary interest less direct, there is simply no such thing as a scientist (indeed, person) without some form of personal view; in science in particular, these views may be deeply held and at times not fully understood in their personal expression by the individual themselves. Scientists have a direct stake in their own theses, the fate of their own ideas vis-a-vis other competing ideas, and in the possible ideological implications of all of these ideas.

Thus, the presumption that there is such a thing as a disinterested or "uncompensated" scientific "peer reviewer" is simply false. Probably no other body of contested fact (which is after all, what science is all about) makes or even pretends to make the assumption that an annonymous source is in the first place credible as a basis for decision, and in the second place that "disinterested" parties even have a proper place in the proceeding. Criminals have a right to be faced by their accusers. Courts admit parties to proceedings only if an interest demonstrably exists. Administrative law proceedings require clear demonstration of interest, and inherently presume that without such an interest any particular party would not be present. Opposing parties present witnesses (whose names are known) and even where the facade of "neutrality" or "scientificality" on the part of the witness is claimed, the presence of such witnesses is allowed only if they have a material effect on the outcome of the matter at issue.

The lack of named individual responsibility may be the single biggest error of existing funding programs, in all facets. In the absence of the kinds of protections our legal system has evolved in all other applications, the forms of abuse the legal system normally protects against abound in science funding. The weirdest and most byzantine perversions of process can and regularly do appear as reviewers comments in "peer reviews" and as outcomes of funding decisions. The effect on the scientific establishment closely resembles that which a one party totalitarian state has on free expression and the distribution of personal favors.\1.

Thus, the single most essential reform is to require, at all stages of whatever funding process exists, that all parties who have any part in or influence over that process be openly named. Responsibility for particular funding decisions must be that of an openly named individual. All comments made by any persons in form of "peer" reviews must be signed and the names of the reviewer openly known. Secrecy has no place in public science decision making and certainly none in science agency project reviews (limited areas of national security excepted).

--Employment Relations.

Closely allied to problems of individual responsibility are those of conflict of interest due to beneficial institutional relationships (ie, relationships of individual agency officers to particular institutions who may offer

---

1. Or just as pernicious, consider this quotation from a past President (1969 to 1978) of one of the Illinois State Universities: "Sincerity was the credo in the seventies; competence was seen as hypocritical". (Illinois Times, page 8, March 28,1985 quoting Robert Spencer). This is a surprizingly candid statement from someone presumably in a position to know whom may have been hired and why at at least one institution. Are these, then, the scientific "peers" ?

them potential material benefit as a direct or indirect result of thier actions). While all other areas of government must guard against establishing special relations, and have in some cases extremely protective exclusion rules for subsequent employments of agency officials, the science agencies specifically hire, into the most key decision making positions, individuals who have beneficial interests in the very institutions to whom they must allocate funds. This occurs particularly in rotational positions for program directors, but also occurs elsewhere in the agencies.

There is a close relationship between the ability of an individual to undertake personal and intellectual risk and their ability to make wise decisions for scientific purposes. This is unfortunately not a forum for addressing the errors of academic tenure mechanisms. However, if the academic institutions follow self-debilitating personnel policies, the federal government is not therefore required to follow suit. Persons who become employed by federal agencies in decision making positions, including program directors, can not be permitted to retain beneficial ties to the institutions to which they may be awarding monies. The existing rotational system maximizes conflict of interest.

--Funding of Awards and Overhead Rates.

Existing practice, and the bulk of the Task Force Report, talk of "individuals" and "institutions in which they work" as if these were inseparable. Making this presumption, many of the problems on determination of "overhead rates" and like questions found by the Task Force necessarily follow. However, if the presumption is not made, then different answers to the administration and funding questions follow.

I do not comment on direct awards made by Congress to particular institutions. However, within the competitive award process, particularly if it resembles the existing process, there is no need to presume that the scientist responsible for conduct of a body of research (for simplicity here I'll call this person the Responsible Principal Investigator, or RPI) is inherently linked with any particular institution for the conduct of a body of research. Indeed, in any given case the necessity for the link must be demonstrated, and the successful demonstration of this link should be presumed a rare event.

Instead what should happen is that all applications would list only the name of the RPI and of any other individual beneficiaries of the proposed award . Once a scientific decision on the merits of the award is made, then a totally separate decision should be made on the administration of the award. The RPI should be required to submit or have submitted separate bids (ie from separate potential administrators) to the funding agency administrative bureau. (If practice allows this to occur simultaneously with initial submission of the award application, then the materials describing administration should be kept sealed, until after the scientific decision is made). Rules should permit the bids to include the possibility that the individual themself will administer the funds or provide support services, that some academic institution will administer or provide these services, or that some other entity will do so. The awarding agency should generally be required to award administrative services to the lowest bidder. The agency and the successful RPI should have the right to seek additional bidders if not satisfied with the initial bids. But the primary responsibility for decision of placement of administrative rights should fall to the RPI.

I'll not here attempt to describe a complete manual of how this process would work. However a few examples of the possible outcome are enlightening. One likely administrative bidder could be a business services department of a commercial bank. I once enquired of a bank their cost to administer such an award, and was quoted a cost which was a fraction of the interest the funds would earn on the average daily balance. Thus, among the possible bids could be amounts administrators would PAY TO the funding agency for administration rights. Other private competitive commercial enterprises would also be interested in more complete packages of support services. For example, shared office complexes could bid to provide office space, common computer facilities (ncluding use of micro-computers at extremely minimal including zero $ hourly rates) and other common facilities. Academic institutions could compete for such bids, and it would not be unusual for them to compete successfully for provision of specialised services such as library use.

--Other Comments on Funding.

The above has presented two concrete proposals which I believe are vital to developing a healthy science policy. I do not minimize the ramifications - policial, economic or scientific - of these proposals. On other occasions I have provided analyses of these effects to other forums. I would be happy to provide such further analysis for your Task Force or Committee.

3. Congressional Independent Judgements About Science.

The next paragraphs of this letter discuss an answer to the question posed at page 58 of the Task Force Report, How Can Congress Make Independent Judgements about the Quality of Past, Present and Future Research?

What the question calls for is an intellectually independent, unbureaucratic, lean and mean, and plain feisty group of thinkers, who are totally unattached to academic institutions, existing agencies or bureaus, and unbeholding to the federal system of science rewards and punishments. They must however be responsible to the Congress and in particular to specific committees of the House and Senate. The solution is a system of what I shall call "Chartered Institutes". I suggest a system of such Institutes, acting under contract to Committies of Congress and coordinated by a single central Institute (hereafter denoted as the "Institute"). While the Institute and the "sub-institutes" would be contractually responsible to the Congress, employees of the Institute would not be staff members of the subcommitties nor direct employees of Congress.

The Institute would have contractual responsibilities to provide certain reports and assessments to specified committies of Congress at certain regular times. The Institute must have an ability to control its own agenda to some large degree, but could not do so to the detriment of its contractual obligations. These Institute or its sub-institutes would not be research agencies, in the sense of funding research programs, and would not operate laboratories. They may require the ability to contract for certain forms of work, and to for example hold scientific conferences and do other acts inherent in the performance of their contracted duties. However, they would in no manner be able to speak for the Congress nor any part of the Congress. They could not hold "hearings" in lieu of or in the name of the Congress or any of its committies. They would not be planning bodies nor administrative agencies in any form. They should not be governmental agencies, and would not be part of any governmental administrative or personnel or civil service system.

Essentially, the staff of the Institute would be hard nosed scientific philosophers, committed to political democracy, and willing and able to undertake intellectual risk when real personal consequences (such as their jobs) may depend on the continued competent performance of their work. In particular, persons employed by the Institute would not be permitted to hold concurrent employment with any scientific institution and in particular would not be "rotations" from for example tenured posts in the academic world.

Funding ~~from~~ for the Institute could come from the regular annual budget of the Congress. However, an alternate means of funding should at least be considered. This alternate means would be to have the funding come from a portion of the net earnings on certain funds of the Federal Financing Bank. I can conceive of several contractual devices whereby this could be possible; for example, the Institute Charter (ie contract) could actually be one between the FFB and the

Institute, requiring the performance of certain duties for the Congress. But the specific instrument used is for the moment less important than the reasons for suggesting this funding source. First, using the FFB net earnings as source of funds would (hopefully) make the Institute less subject to the most immediate short run pressures which could affect the outcome of its reports. Secondly, as these earnings may be subject to somewhat normal economic risk, the Institute would have less of a tendancy to believe its resources (which otherwise would be the Federal budget) were essentially unlimited. However, at the same time it may be possible to obtain continuing Institute funding for more than a one year period; this would not only relieve certain pressures from the Institute, it would considerably simplify its administrative structure and need for a complex administrative bureaucracy within the Institute itself.

However, as I noted the specific funding mechanism is a bit less important that the creation of the Institute itself. Nor for that matter do I forget that many other important institutional questions would need to be answered in process of creating the Institute. As in other areas outlined in this letter, I would be happy to discuss these concepts further and in depth.

4. Conclusion.

Your Task Force has identified vital questions for science policy. If a proper opportunity exists, I am interested in addressing many of the questions posed by the Report of the Task Force. The present letter however addresses certain specific questions in which I have maintained a long standing interest and public position. I hope you find these a useful contribution to the efforts of your Task Force.

Sincerely,

PABallonoff

Paul Ballonoff

mailing address:

734 A Bruns Lane
Springfield, Illinois 62702

Phone contact: (217) 787-1592
(217) 785-4046

Resume: Paul Ballonoff
Senior Economist
Policy Analysis & Research Division

Employment and Business Address: Illinois Commerce Commission
527 E. Capitol Avenue
Springfield, IL 62701

Phone: (217)785-4046

Principal Duties:

Analyze and testify on pricing structures, costs and operations of wholesale utilities operating in or affecting Illinois. Manage ICC interventions into Federal Energy Regulatory Commission rate cases.

Previous Experience (Reverse Date Order):

January 1984 to January 1985: Operations Research Analyst for the Kansas State Corporation Commission; August 1980 to January 1984: Rate Analyst for the Kansas State Corporation Commission. Analysis of, field audits on, and expert witness testimony in state and federal gas and electric rate cases, including Northwest Central Pipeline Co.; Northern Natural Gas Co.; Panhandle Eastern Pipeline Co.; Getty Gas Gathering Co.; Kansas-Nebraska Natural Gas; Union Gas System; Peoples Natural Gas Co.; The Gas Service Co.; Kansas Gas and Electric Co.; Smokey Hill Electric Coop. Independent original research on utility cost and pricing structures. Testimony on issues including cost allocation and rate design of gas or electric wholesale and retail utilities; zone separations of costs; gas transmission and distribution rates; gas costs; gas reserves management; gas innovative rate designs; cogeneration; depreciation.

1976 through 1984: Independent Consultant in Economics and Organizational Economics. Services included study of the Missouri State Prison Farm system; of the retirement system of the Texas Municipal Power Agency; of the economic impact as of certain Federal monetary and fiscal programs and their operation. Testimony to several Federal executive agency hearings on national economics policy.

1970-1976: Academic Career: Assistant Professor, Southern Illinois University, Carbondale, Illinois, 1970-71 (teaching general Anthropology, Ethnological Theory, graduate course on mathematical social models). Assistant Professor, Center for Demographic and Population Genetics, University of Texas, Houston, 1972-76 (research position in human social and population structures). Associated teaching posts at Rice University (Houston, TX 1972-1976), University of Texas School of Public Health (Houston, TX 1972-1976) and University of Washington (1974-1975).

Other Relevant Experience:

1964-1966: Peace Corps Volunteer, rural development, Colombia, South America, development of rural economies and small business; instruction in local law and accounting for small business institutions.

1967-1970: Course work and field experience/training in socio-technical systems studies applied to governmental institutions and corporate structures, UCLA Graduate School of Management.

Education and Training:

BA in Economics, emphasis on quantitative and theory, UCLA, June 1964. Ph.D. in social anthropology, UCLA, June 1970 major field social theory, minor fields human ecology and mathematical methods, thesis on mathematical theory of culture. Post doctoral study in mathematics for social sciences, University of Illinois, Urbana, 1971-72. Special studies courses in quantitative methods for social sciences, Williams College, 1968 and in population genetics, University of Colorado, 1971. Special courses and professional meetings participation in utilities regulation and economics; cogeneration; solar energy; population and social theory; cybernetics and systems research.

Special Items:

Recipient of State of Kansas Employee Suggestion Award for micro computer application to rate design and cost analysis (1983).

Member of American Arbitration Association Commercial Panel of Arbitrators.

Delivered invited recent lectures to Austrian Society for Cybernetic studies (1982), Kansas State University (Sociology, Anthropology Dept - 1983); University of Southern California (Anthropology Department - 1984); Economics Department - 1984); University of California, Irvine (Mathematical Social Sciences Group - 1984).

Publications:

Books: Paul Ballonoff is author or editor of five published books in mathematical social sciences. The titles and dates of edited books are: Mathematical Models of Social and Cognitive Structures (1974) University of Illinois Press; Genealogical Mathematics (1974) Mouton (Maison des Sciences de l"Homme); Genetics and Social Structure (1974) D H & R Publishers; Demographic Genetics (1976) D H & R Publishers. Titles of sole authored works are Mathematical Foundations of Social Anthropology (1976) Mouton (Maison des Sciences de l"Homme) and, under preparation, Mathematical Theory of Culture.

Other Publications: Paul Ballonoff has published technical articles in professional journals including Advances in Cybernetics and Systems Research, Social Science Information, Synthese, American Anthropologist, American Journal of Physical Anthropology, American

BEST COPY AVAILABLE

Indigena, and Journal of Economic Behavior and Organization, regulatory from 1970 to present; and popular press articles in newspapers or other journals.

Recent articles include:
"Demand Layered Costing and Pricing of Electricity", in Public Utilities Fortnightly, March 3, 1983; "Elasticity Properties of Rate Design Subject to a Fixed Revenue Requirement" mimeo, 1982; "Mathematical Theory of Social Demography" in Progress in Cybernetics and Systems Research, Vol. 10, page 101-112, (1982); "Mathematical Theory of Social Demography, II", (1982) Trappl (ed.) Cybernetics and Systems Research (1982); "Theory of Lineage Organizations", American Anthropologist March, 1983.

SUMMARY OF DOCKETS IN WHICH PAUL BALLONOFF HAS PARTICIPATED:

I. RATE CASE TESTIMONY	Major Issues*	DOCKET NO.
Kansas Gas & Electric Company	R, COS	128,139-U (KCC)
Kansas Gas & Electric Company	R, COS, JS, D, O	134,792-U "
Gas Service Company	R	126,922-U "
Gas Service Company	R, COS	132,996-U "
Gas Service Company	R, D	136,372-U "
Gas Service Company	R, COS, T, PP, O	136,850-U "
Union Gas System	R, COS	126,132-U "
Union Gas System	R, T	131,884-U "
Peoples Natural Gas	R, COS, D	127,137-U "
KN Energy	R, COS	124,127-U "
KN Energy	R	132,202-U "
KN Energy	F	106,850-U "
Smokey Hill Electric Cooperative	R, COS	130,148-U "
Midwest Energy, Inc.	R	133,889-U "
Panhandle Eastern Pipeline Co.	R, COS	RP 82-58 (FERC)
Panhandle Eastern Pipeline Co.	PP, F	TA 84-1-28 (FERC)
Northern Natural Gas Co.	PP	RP 82-71 "
Northwest Central Pipeline Co.	R, COS, O	RP 82-114 "
II. GENERIC TESTIMONY:		
Co-Generation Section of PURPA	R, O	115,379-U (KCC)
KCC Generic ECA Docket	R, PP, F, P	140,728-U
KCC Generic Gas Deregulation	T, PP, F, COS, R, O	142,554-U "

III. OTHER PARTICIPATION IN DOCKETS:

Northwest Central Pipeline (Settled)	R, COS, JS, PP, O	RP 82-114 (FERC)
Kansas Gas and Electric (Settled)	R, COS, JS	ER 82-412 "
ARKLA Gas Co. (Settled)	COS	RP84-107 "
KN Energy (Pending)	COS, O	RP85-11 "
Getty Gas Gathering (Withdrawn)	R, COS	135,143-U (KCC)

*Code: R = rate design; COS = allocation of costs to classes; JS = jurisdictional separation of costs; D = depreciation; T = transportation tariffs; PP = purchasing practices, O = other; F = fuel or energy cost.

COMMENTS ON
THE ROLE OF THE SOC.AL AND BEHAVIORAL SCIENCES

FOR
TASK FORCE ON SCIENCE POLICY

PREPARED BY
PAUL ALAN BALLONOFF

In the December, 1984 Report, the Task Force correctly noted that the social and behavioral sciences may have a special role to play. The Task Force noted that this role in unique in no small part because the "findings" of these fields of study border "on matters where social, political, and religious values are important; and the promise of improving human behavior and social life on a substantial scale is offered ...".

Since the Task Force seeks to know future possibilities which may implement this promise, as well as critical analysis of the past performance, let me start with the possible future before discussing correcting past errors.

Let me start with a parallel from the physical sciences which in a general way describes the social science subject I will discuss. Broadly speaking, there are two complementary ways to view the physical world: statistically, through what is known generally as statistical mechanics (or thermodynamics); and structurally, through what is known generally as quantum mechanics. Historically, the statistical view arose first and developed into a modern science, with much of the modern theoretical apparatus by 1900. Quantum theories, which made possible a great portion if not all of modern knowledge of the structure of the atom, electronics, and so forth, was created essentially after 1900, principally in 1920 to 1930.

This is not to say that quantum concepts do not have statistical implications, they most assuredly do. However, with a workable quantum mechanical theory, one can analize the structure of an object and infer from that structure a good deal of knowledge about the properties that thing has or will have in certain circumstances. To do this, technically, quantum theories are very different mathematically than are thermodynamic theories. In fact, there are some serious problems in reconciling technically and philosophically between the two approaches, in spite of the fact that both deal with testable (and tested) concepts.

Now I am going to inform you of something interesting: something very like quantum mechanics is possible for the social sciences. In fact to an extent, it already exists, but is not well known. And, by the way, I do not mean this in the figurative sense of "lets take a quantum leap into the future", and I am not saying something about taking some quantum stuff from physics and applying that to the organization of IBM. I am saying that a purely social science theory exists, which differs from purely statistical social studies in the same way that physical quantum theory differs from statistical mechanics. A technically and philosophically different theory which has different but useful, testable, and scientific results.

Essentially what this new theory does is let you look at the rules of cultural bonding, and infer what kinds of measures will be observed on a system that acts with those rules. Or, if the rules change, the theory says what happens to the measures, and perhaps what other consequences occur in the process. These "measures" and "consequences" include things like population size, growth rates, decline risks, sizes of local groups, times of fissioning of communities into separate communities, conditions at which different cultures are in non-growth states, relations between social histories and population histories, and so forth. And, these are simply technical possibilities of the portion of that theory which already exists.

The applications of this kind of work are very broad. Does every culture reach zero population growth at "two children per couple"? (No!) Can societies be stable with large average family sizes even with good medicine ? (Some could, yes.) If an emergency causes forced refugee resettlement, how big should the resettled communities be? If the old practices of village life are changed, what does this do the growth rate, family size, village size? (Answers to both questions depend on the cultures of the system). If this theory can interpret history, can it predict future paths? (Yes, "conditionally", ie it can answer questions in the form "if the future culture is such-and-such the the future measures would be thus-and-so", and also in the form "which cultures would be expected to have thus-and-so measures". It can also answer questions on the measures that would be found in the transition periods between.)

Now, I happen to know about this theory because I created it, and I have tested it on real reported cultural data, and I have published it in books and scientific journals both in this country and abroad, though mostly abroad in Europe. In fact I could tell you stories about how our American scientific establishment treats new science that would sound very like a Soviet "dissident" dealing with their bureaucracy. But I came here today to be constructive, so instead what I will discuss are some answers to the other general questions of the Task Force. I shall do so from the view of a "quantum theorist" who has spent his career dealing with a bureaucracy controlled by convinced statistical mechanical believers, and it is not yet 1930.

While the Task Force presented its questions in a value-neutral way, the questions themselves are in no form value-neutral. Thus my first point is that, no matter what may result in the structure of funding for social and behavioral (or indeed any) sciences, we must be absolutely certain that the way "findings" of projects are used does not pre-empt the political process. However brilliant, technically trained, sophisticatedly computerized (or indeed, arrogant) may be those who perform the studies, the fact is that these people have no special Constitutional role to play in the political process, no special standing which gives them a higher right to assert their values or "findings".

One of the failures of the existing system of science funding has been failure to recognize that the rights of persons (corporate and individual) within the academic and science funding systems are no different from the rights of other citizens and legal persons. Believing scientists to be somehow less or more (or in-?) human, existing science policy has failed to apply the same rights and the same presumptions about the exercise of those rights as exist throughout the remainder of our legal system.

There is no other place in the structure of our government where quasi-secret societies allocate tax monies with no specific responsibility for who receives funds, or why, no accounting for the results of the expendures, and no individual named with specific responsibility for each part of the process. The possible exception may be aspects of national security and intelligence activities. Can anyone seriously claim that the member of a scientific "peer review" committee requires the same protections of identity as does an intelligence agent?

Instead, and precisely because individual values can so strongly influence choice and action, we must assure that there is no place in the science funding and review process for people who are unwilling to sign their name to their work. The presumption that scientists on review boards and panels give their time with no hope of reward is simply absurd. This presumption is the precise opposite of that applied in nearly any other body of administrative law, or court practice. Indeed, if a person has neither knowledge of nor interest (in a material and legal sense) in a proceeding, they have no purpose in that proceeding. It is precisely because "service" on "peer review" committies provides such intimate knowledge of exactly what ones competitors may be doing, and such relative power to influence or disrupt those doings, that people are willing to "serve" on them at all.

Closely related to the principle of openly named participation is the principle that those who make the administrative decisions within an agency must have no material conflicts of interest with the entitles to which they may be awarding funds. The present system of "rotating" persons from academic institutions into the administrative positions of funding agencies maximizes conflict of interest. The result is systematic heavy bias toward the interests of the institutions, whenever there may be a conflict between these and the interests of science as a whole.

Therefore it is imperative that the federal funding agencies or mission agencies develop their own cadre of scientist/administrators, whose loyalty is not to the same bodies to which they are awarding funds. Or, short of this, it must offer science administrators long enough term contracts that they could (and would be required to) resign, and be fully released from, all academic appointments for the term of their service.

Another principle is that centralization of decision making leads to levelization of thought. There is no more certain way to eliminate intellectual diversity and therefore to minimize the possibility of technical progress, than to cause all decisions to be made through a single bureaucracy. It is inevitable that this body will develop its own ideology, and highly probable that it will not even know "consciously" that it has done so; if the ideology is conscious, so much the worse for possibility of progress that differs from that ideology.

But plainly speaking, this is precisely what we have done to the social and behavioral sciences. Strip off the statistical incantations, and where in the social and behavioral studies is the hard analysis, clear minded reasoning, that one has a right to expect from sciences? I will not say there has been none, but there has been far less than we have bargained for, and paid for.

If there is a role therefore for a National Science Foundation type organization, it therefore most certainly must not be as the single funding body for sciences. There is perhaps a role for a single body whose sole purpose is to take the highest degrees of risk.

The existing NSF and NIH organizations avoid risk. Yet the lowest risk areas of sciences are precisely those which should be funded, if at all, through other means. If "scientific statistical research method" is so well established and useful then it belongs in the applied studies of agencies which think they have a use for it. If responsible agencies have not or can not find means in their operating budgets for social/behavioral research and applications, than this of itself tells something very important about the value of that "research".

Thus, Congress should not require each applications agency to include social and behavior science in its budget. It might ask, however, whether each agency has such persons, and how it uses them. It might inquire whether any agency can carry out its statutory mission better with some portion of its employment or contract service, done by social/behavioral scientists. The final test of this however, must never be that an agency has "successfully used" social/behavioral sciences because it has hired a few practitioners. Instead, the test of value must be the hard nosed test of whether ability to complete the agency mission has been improved and perhaps whether its total operating costs are reduced, by such hires or contracts.

In analizing the uses of social sciences in agencies, it is also worth considering whether much of the malaise of the social/behavioral sciences may be due to the widespread existing use of many of their techniques. As this sounds contradictory, let me elaborate at bit. To a very large extent, the things which are taught as "research methods" for the social and behavioral sciences are taught widely to all of them, and taught widely outside of those fields themselves. For example, students of marketing, business or public administration who have never taken sociology or demography, will still have a likely broad training in survey methods, sampling techniques, population analysis, and the like. Thus, we may be badly misleading ourselves to talk of the "need" to expand federal or societal use of social/behavioral sciences, because that expansion is already very broad into many of the areas that may make best use of the methods.

But at the same time, the fundamental institutions, the academic institutions and the federal funding agencies, see themselves as besieged for lack of funds and cramped for research on the same techniques, subjects and methods. Instead of being structured to develop new concepts and techniques, the institutions have become convoluted into continual recycling of the old. This happened precisely because of the ideological, technological, and institutional levelizing effects I discussed above.

If this is true, and I am very certain that it is true, then the problem will not be solved by simply requiring agencies to spend funds for social/behavioral projects. In my view, many of the most important social science possibilities are not even within the technical ideologies of the present institutions.

I appeared today because I believe that the existing system has become encrusted with a levelized and possibly played-out technical ideology. Thus I believe that the single best action Congress could take (besides assuring all participants in the process are openly named) is to diversity; diversify the sources of funds, diversify the means by which they are awarded, diversify the eligibility for receipt of funds, diversify the responsibility for applications of results.

I hope you find these comments useful.

O